NBER WORKING PAPER SERIES

CAN VAT CUTS AND ANTI-PROFITEERING MEASURES DAMPEN THE EFFECTS OF FOOD PRICE INFLATION?

Youssef Benzarti
Santiago Garriga
Darío Tortarolo

Working Paper 32241
http://www.nber.org/papers/w32241

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2024, Revised July 2026

We thank the Editor (Rema Hanna) and four referees for their valuable comments and suggestions. We are grateful to Pierre Bachas, Raj Chetty, Lucie Gadenne, Jarkko Harju, Pascal Michaillat, Clara Martinez-Toledano, Emi Nakamura, Emmanuel Saez, Alisa Tazhitdinova, Tejaswi Velayudhan, Roman David Zarate, and several seminar and conference participants for helpful suggestions and comments. Kuda Maposa provided outstanding research assistance. We acknowledge financial assistance from the Institute for New Economic Thinking, the EUR grant ANR-17-EURE-0001, the World Inequality Lab, CEPREMAP, the Institute for Fiscal Studies, and the University of Nottingham. The views expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent, nor those of the National Bureau of Economic Research.

Can VAT Cuts and Anti-Profiteering Measures Dampen the Effects of Food Price Inflation?
Youssef Benzarti, Santiago Garriga, and Darío Tortarolo
NBER Working Paper No. 32241
March 2024, Revised July 2026
JEL No. H0

ABSTRACT

We study a temporary VAT cut on food, its re-introduction, and anti-profiteering price caps during high inflation in Argentina. Using barcode-level data from over 3,000 supermarkets, we find: (1) in monitored chains, VAT-cut pass-through is near-complete, unlike the incomplete pass-through in less-monitored settings; (2) in unmonitored independents, pass-through is asymmetric—prices rise more after reinstatement than after the cut and stay above pre-cut levels; and (3) caps compress price increases in chains but leave capped prices persistently below pre-cut levels. A consumer surplus model shows the VAT cut was progressive, with caps reducing the asymmetry's regressive impact.

Youssef Benzarti
University of California, Santa Barbara
Department of Economics
and NBER
youssef.benzarti@gmail.com

Santiago Garriga
CEFIP-UNLP
garrigasantiago@gmail.com

Darío Tortarolo
The World Bank
Development Research Group (DECRG)
dtortarolo@worldbank.org

1 Introduction

Inflation has long posed critical challenges for Low- and Middle-Income Countries (L&MIC), where high and sustained price increases often trigger significant economic and political instability. This issue has gained renewed urgency in the wake of a sequence of global shocks — from the COVID-19 pandemic to the war in Ukraine and disruptions to global energy markets — which have fueled inflationary pressures across much of the developing world. In response, L&MIC governments have relied on a mix of conventional tools (such as raising interest rates) and unconventional interventions, including price controls and consumption tax cuts. While conventional policies have been extensively studied, the effects of unconventional approaches remain far less understood, especially in L&MICs.

One increasingly common policy, adopted in both L&MICs and OECD countries, has been the temporary reduction of Value-Added Tax (VAT) rates, with the hope that it would lower consumer prices and shield low-income households from inflation. By our count, at least thirty-one L&MICs have adopted such temporary VAT cuts in recent years (see Appendix Table A1). These reduced rates are a prominent example of a *tax expenditure*—a statutory departure from the benchmark tax system—and rank among the largest and least-scrutinized sources of forgone revenue in developing economies. Yet rigorous causal evidence on whether they reach their objectives, and at what cost, remains scarce. What evidence exists, mostly from high-income settings, suggests that VAT cuts are passed through to prices only incompletely, benefiting firms more than consumers. This raises a compounding concern at repeal: if firms pass through more of the VAT increase than they did of the cut, prices can settle above their pre-cut levels.[1]

This paper studies Argentina's 2019 temporary VAT cut on basic foodstuffs, paired with anti-profiteering measures to limit price increases upon its repeal. From August 16 to December 31, 2019, the reform zero-rated (cut the VAT from 21% to 0%) a subset of items from the official food basket used to compute the cost of living. Otherwise similar food and non-food products remained taxed at the standard rate, so that the VAT was removed on tea, milk, eggs, and rice, for instance, but not on coffee, breakfast cereals, or crackers. The policy followed a surprise presidential primary election result that led to a sharp depreciation of the peso and raised concerns about food affordability—a context in which Argentina's chronic inflation and repeated reliance on conventional stabilization tools made such an intervention particularly salient.

When the VAT cut expired and the standard 21% rate was reinstated, the government did not allow prices to adjust freely: in supermarket chains, it selectively applied anti-profiteering measures to a subset of the affected goods, limiting the scope for prices to overshoot their pre-cut levels. For some products that had been zero-rated, authorities capped post-repeal price increases at no more than 7% above their December 31, 2019 levels. Some items faced stricter controls—such as liquid

[1] See Kosonen (2015), Benzarti & Carloni (2019), Harju et al. (2018b), Benzarti et al. (2020) for examples of such evidence and Benzarti (2025) for a review of this literature.

milk, which was required to remain at pre-repeal prices—while others, like organic rice, were not subject to any caps. These measures drew on pre-existing legislation empowering the government to control and monitor grocery prices, and were enforced by the same agency responsible for antitrust regulation using a real-time price monitoring system launched in 2016. Firms found in violation could face fines and other sanctions, and no expiration date for the caps was publicly announced. Enforcement ultimately ceased with the onset of the COVID-19 pandemic in March 2020.

The novel combination of a temporary VAT cut and the selective application of anti-profiteering measures upon its repeal provides a compelling setting to study how VAT changes pass through to consumer prices, and how anti-profiteering measures reshape that pass-through. We exploit this variation using a dynamic difference-in-differences design that compares outcomes across three groups: (1) goods treated by both the VAT cut and the anti-profiteering caps; (2) goods affected only by the VAT cut and free to adjust prices upon repeal; and (3) a control group of goods unaffected by either policy. Our identification relies on the standard parallel trends assumption—that, absent the VAT reform, prices of treated and control goods would have evolved similarly, which we verify holds across all three groups prior to the reform. We also address the possibility of spillovers from the treatment to the control group using two alternative strategies, and conduct a battery of robustness checks to validate our findings.

Our analysis relies on two main data sources. First, we use high-frequency barcode-level scanner data from Scentia, a market company that collects prices and sales volumes directly from over 3,000 grocery stores—weekly for chain supermarkets and monthly for independent supermarkets. The dataset spans January 2018 to June 2021 and, for each barcode, we observe VAT-inclusive prices, product descriptions, and quantities sold. The "independent supermarkets" category does not include small corner shops or convenience stores ("mom and pop" stores), but instead stand-alone supermarkets and small regional supermarket chains. Street vendors and other informal retailers account for a very small share of food expenditures in Argentina, representing at most 2% of zero-rated-good purchases across the income distribution (see Section 5.2); their limited prevalence contrasts with the settings studied in Bachas et al. (2023), which focus more on lower-income countries. Second, we also use detailed expenditure microdata from the 2017–2018 National Household Expenditure Survey to further assess the distributional effects of the VAT changes.

Overall, we find that monitoring and anti-profiteering measures can make a VAT cut reach consumers and blunt its asymmetric rebound at repeal—but that this success was uneven across stores and came with distortions of its own. Pass-through differs sharply by store type, a margin that matters for incidence because households of different incomes shop in systematically different stores, echoing how the burden of consumption taxes depends on where purchases take place (Bachas et al., 2023). In chain supermarkets, where the government monitored prices and announced anti-profiteering measures upon repeal, the pass-through of the VAT cut is near-complete, indistinguishable from the full pass-through benchmark of 17.4 percentage points, once we account for substitution across goods and a concurrent peso depreciation. This contrasts with the previous literature on VAT cuts, which typically estimates pass-through well below 100% in less-monitored settings. In independent supermarkets, by contrast, prices respond asymmetrically and replicate that previous-literature pattern: roughly 35% of the VAT cut is passed through to prices, while the subsequent and equal-sized VAT increase passes through more substantially, leaving prices above their pre-cut level.

We also find that the anti-profiteering caps reduced the price response to the VAT increase among capped goods in chains. First, capped goods treated by the VAT cut rose less after the increase than otherwise-similar treated goods that faced no caps. This price gap persists for at least one year after the VAT increase. Second, we provide evidence that these price caps were binding: when plotting the distribution of price changes one week before and one week after the VAT increase, we observe significant bunching at the 7% cap. Some prices, nonetheless, increased by more than 7%, suggesting imperfect compliance. This is likely due to the difficulty of monitoring percentage price changes rather than nominal price levels. Indeed, we show that goods subject to nominal price controls (i.e., 0% price change caps) are much more compliant with these caps: their average prices remained constant (despite high inflation).

These caps compressed the price response of capped goods to the VAT increase in chain supermarkets by roughly a factor of two; prices in independent supermarkets, which were not monitored, continued to respond asymmetrically. The chain–independent gap in capped goods is partly inherited from differential pass-through during the VAT cut: the caps, whose purpose was to limit price increases at the repeal, kept chain capped-goods prices below their uncapped counterfactual, sustaining the gap rather than creating it. Holding chain prices below the level they would otherwise have reached is itself a form of distortion: capped goods in chains settled persistently below their pre-VAT-cut levels relative to control, an asymmetric response in the opposite direction.

These price effects carry over to consumer surplus. Estimating the consumer-surplus effects of the VAT cut together with the anti-profiteering measures, we find that the measures led to substantial gains relative to a simulation without them. This surplus was, however, unevenly distributed, and two distinct comparisons are easy to conflate. Taking the reform as a whole, higher-income households benefited more, because pass-through was near-complete in the chain supermarkets where they shop but asymmetric in the independent stores where low-income households concentrate. The anti-profiteering caps, considered on their own, cut the other way: relative to a simulation with no caps, they benefited low-income deciles disproportionately, because they blunted a regressive VAT-increase shock that would otherwise have fallen hardest on those deciles. We develop both comparisons in Section 5.

One important concern with our analysis is the potential for bias arising from consumer substitution between treated and control goods. For example, if the price of tea decreases due to the VAT cut, some consumers may switch from coffee to tea, increasing demand and potentially raising the price of tea—thereby biasing our estimates. We address this concern using two main approaches. First, while some treated and control goods are plausible substitutes (such as tea and coffee or sunflower and olive oils), most are not (see Table 1). Many control goods, including breakfast cereal, salt, herbs, and jams, lack close substitutes in the treatment group, which mitigates this substitution concern. In fact, when we re-estimate our main effects after removing obvious substitutes from the control group, such as coffee for the case of tea, we find quantitatively similar pass-through rates. Second, we repeat the analysis using an alternative control group composed of non-food items (e.g., cleaning products), which are highly unlikely to be substitutes for the food items affected by the VAT cut. With this cleaner control group, the estimated pass-through of the VAT cut in chains rises and approaches the full pass-through benchmark. Taken together, substitution operates in the expected direction and biases our baseline estimates modestly downward; correcting for it—most cleanly with the non-food control—moves chain pass-through close to full.

Another identification threat is the depreciation of the peso, which occurred just before the VAT cut. The concern is that this shock may have affected treated and control goods differently, thereby confounding our estimates. Using another peso depreciation shock in 2018, we show that the prices of basic necessities targeted by the VAT cut indeed rose more than those in the control group. However, this differential is relatively small and implies that, absent the depreciation, our VAT pass-through rates would have been 1.4 percentage points larger. In chain supermarkets, where the response to the VAT cut is largest, combining this depreciation adjustment with the substitution correction discussed above brings the estimated pass-through of the VAT cut closer to the full pass-through benchmark of 17.4 percentage points.

This paper's main contribution is to show that governments can shape the incidence of VAT changes through monitoring and anti-profiteering measures—though not without introducing distortions of their own. This is particularly relevant because several L&MICs have been struggling with high levels of inflation in recent years and have used VAT cuts to mitigate the burden on consumers. However, few L&MICs have also implemented concurrent anti-profiteering measures. As these VAT cuts are repealed, prices may rise sharply, often more than they fell during the initial VAT reduction, because VAT changes tend to be passed through asymmetrically when anti-profiteering measures are not in place, as we document. This dynamic can undermine the intended benefits of temporary VAT relief by reducing real purchasing power in the medium run. We show, however, that deploying anti-profiteering measures upon repeal, alongside a price-monitoring infrastructure, can compress the asymmetric pass-through of VAT increases, although this came at the cost of holding capped-goods prices persistently below their counterfactual — an asymmetric distortion in the opposite direction that we return to in Section 4.4 and the Conclusion.

More broadly, we contribute to the tax incidence literature. A closely related paper is Bachas et al. (2023), who show that the incidence of consumption taxes can be different in informal "mom and pop" shops because these types of stores are more likely to evade them (unlike our setting, where such stores are rare, representing less than 2% of total consumption of the basic food items we consider).[2] We also contribute to a literature discussing VATs as a policy tool governments could

[2]See the following for examples of articles studying the incidence of consumption taxes, the majority of which

use to affect the economy, in this case using VAT cuts and anti-profiteering measures to affect prices in times of high inflation (see Blundell, 2009; Crossley et al., 2009, for example). D'Acunto et al. (2022) and Bachmann et al. (2026), for instance, consider the suitability of VATs as an alternative to conventional fiscal policy, especially in times when nominal interest rates are close to zero. Our paper shows that, while such policies can be effective at lowering prices, they can have unintended consequences even when paired with anti-profiteering measures.

While the joint implementation of VAT changes and anti-profiteering measures is a relatively rare and novel policy, it is not unique to Argentina. Similar approaches have been adopted in other L&MICs: India introduced anti-profiteering measures in 2017 to ensure that reductions in the Goods and Services Tax were passed through to consumers rather than captured by firms, and Malaysia is another example (OECD, 2024). Several OECD countries have implemented similar anti-profiteering measures when adopting a VAT, including Germany, the Netherlands, Korea, Belgium, and Ireland (see Tait, 1988). These measures were more common in OECD countries during earlier periods of high inflation and have largely fallen out of use there as inflation ceased to be a pressing concern, while re-emerging more recently in L&MICs facing high inflation. The vagueness and discretionary enforcement of Argentina's policy mirror how these measures are often deployed elsewhere, as discussed in Section 2.3. We believe Argentina's experiment offers useful insights for other L&MICs that have implemented or are considering temporary VAT cuts, particularly during periods of economic distress. Our paper helps inform the resulting debates—about how to manage the expiration of temporary VAT cuts and whether complementary measures such as anti-profiteering caps can shape price dynamics—by documenting both their intended and unintended effects.

Our findings have important policy implications for L&MICs. First, in contrast to the previous literature on consumption tax incidence, close to full pass-through of VAT cuts is possible, as is the case with chain supermarkets in this paper. However, independent stores, which were not subject to monitoring or anti-profiteering measures, exhibit pass-through behavior consistent with the findings of previous VAT-cut pass-through literature. We posit that this reflects the absence of monitoring in independent stores, which aligns them with the less-monitored settings of the previous literature. Second, we show that it is possible to use anti-profiteering measures to affect the pass-through of VAT changes to prices. However, this tool can lead to additional price distortions, thereby increasing efficiency costs. Third, our results speak to the growing debate on tax expenditures. A reduced VAT rate on food is frequently defended as a pro-poor instrument, but our evidence warrants caution: it forgoes substantial tax revenue while delivering pass-through that is neither automatic nor distortion-free, even when paired with active enforcement.

are from OECD countries: Sidhu (1971), Chouinard & Perloff (2004), Delipalla & O'Donnell (2001), Anderson et al. (2001), Doyle & Samphantharak (2008), Kopczuk et al. (2016), Poterba (1996), Kosonen (2015), Gaarder (2018), Carbonnier (2007), Besley & Rosen (1999), Genakos & Pagliero (2022), Buettner & Madzharova (2021) and Fuest et al. (2024). Kotlikoff & Summers (1987), Fullerton & Metcalf (2002) and, more recently, Benzarti (2025) provide a survey of the tax incidence literature.

2 Institutional Setting

The main identifying policy variation we exploit consists of a temporary 21 percentage point VAT cut on essential food items, along with anti-profiteering measures implemented when the VAT was reinstated. In this section, we describe the main features of these reforms as well as their political and economic context.

2.1 Macroeconomic Context and VAT Holiday

The VAT change took place in a context of high inflation ($\sim$ 55% annually in 2019), presidential elections, and a sharp depreciation of the Argentine peso. On August 11, President Macri lost the primary presidential election to the left-wing candidate Fernandez by a 15.5 percentage-point margin, which was much wider than expected. This triggered a negative market reaction the following day, leading to a 24% depreciation of the Argentine peso relative to the U.S. dollar.[3] Three days later, on August 15, the government implemented a 4.5-month-long VAT holiday on basic food, with the official goal of containing the impact of the peso's depreciation on prices (Executive Order 567/2019). As a consequence, the VAT cut was entirely unexpected. It was also announced on that day that the VAT cut would be temporary, and that it would be repealed on December 31, 2019. According to the Minister of Finance, the fiscal cost was projected to be 10 billion pesos; equivalent to USD 160 million, or roughly 7% of monthly VAT revenue (equivalently, 0.6% of annual VAT revenue).[4] The revenue loss due to the VAT cut was funded using budget reallocation.

The tax rate was reduced from 21% to 0% on a list of 13 goods from the Basic Food Basket, while other basic food products remained taxed at the standard 21% rate. The Basic Food Basket is used to compute the Extreme Poverty Line and is part of the Consumer Price Index used to measure inflation. All the goods analyzed in this paper are normally taxed at the 21% standard VAT rate, except for wheat flour and bread, which are taxed at the 10.5% reduced rate. According to the National Institute of Statistics (INDEC), the categories with temporary 0% VAT accounted for 26% of total food expenditure. The selection of goods targeted by the VAT cut was based on prevailing consumption habits in Argentina, a practice that is consistent with similar food-based VAT interventions in other countries (see Appendix Table A1). The Argentine Government explicitly stated, in the reform's legal text and in several official announcements and public statements by senior officials to the press, that it was targeting goods commonly consumed by Argentine households as part of the basic food basket.[5] Only three days elapsed between the primary election and

[3] See Appendix Figure E.1. For more details, see this NY Times article. Table A1 also presents significant double-digit food inflation rates in countries that have experienced temporary VAT reductions recently.

[4] The revenue forgone associated with this reform was calculated at 0.07% of the GDP, based on *ex-post* official estimations (Source: *Subsecretaría de Ingresos Públicos. Secretaría de Hacienda, Ministerio de Economía*). We provide additional details in Appendix Section C.

[5] Executive Order 567/2019, which legislated the VAT cut, motivates the reform by stating that "*given the prevailing economic and social context, it is considered necessary to establish that the sale of certain products of the food*

the implementation of the VAT cut, which left limited room for a more granular targeting exercise.

The VAT cut only applied to sales made to final consumers, and supermarkets could claim back any VAT credit generated from purchases from suppliers or use it to offset other tax liabilities. Technically, treated goods were taxed at a 0% rate rather than exempted from the VAT; this is important because firms cannot deduct the VAT on the intermediate inputs of exempted goods, whereas they can for zero-rated goods. The left panel of Table 1 shows the list of goods treated by the temporary VAT cut and, in the right panel, a list of untreated goods that are also part of the basic food basket. This table shows that otherwise similar goods were not included in the treatment group. For example, the VAT rate was cut for sunflower, corn, and mixed oils but not for olive, soy, and canola oils. Similarly, the VAT rate for tea and yerba mate was cut, but not for coffee. In our empirical analysis, we use these two groups to estimate price responses using a simple difference-in-differences approach.

2.2 Real-time Price Monitoring in Supermarket Chains

The government was able to monitor prices daily in chain supermarkets, but not in independent supermarkets. This is because, in 2016, the government launched the Electronic Price Advertising System (SEPA, for its acronym in Spanish) to monitor supermarket prices in real time (Resolution 12/2016). This program, popularly known as *Precios Claros* is administered and enforced by the Consumer Protection Office. Its official goal is to enhance price transparency and comparability, allowing consumers to make more informed purchasing decisions—especially important in a high-inflation context where prices change frequently.

In practice, the government provides detailed technical guidelines and software that chain supermarkets must use to report daily price data for each barcode and point of sale. These stores are required to submit their full price list before 6:00 a.m. each day, with a window for corrections until 10:00 a.m. The submitted prices are then published on an online platform accessible via computer or mobile app, allowing consumers to search and compare prices store by store (Appendix Figure E.2 illustrates *Precios Claros'* salience). Participation in *Precios Claros* is optional for independent supermarkets, given the administrative burden involved (Art. 4, Res. 12/2016). In the context of the VAT reform we study, this distinction is important: VAT changes were more easily enforced in chain supermarkets because their prices were subject to daily public and governmental scrutiny.

basket [canasta alimentaria] shall be subject to a rate equivalent to zero percent (0%) of the value-added tax", and lists the targeted categories in its Annex. In a televised announcement on August 14, 2019, President Macri stated that the government would "*eliminate the VAT on the main food products consumed by Argentine families.*" Asked the following day about the criteria used to select the treated goods, Minister of Production Dante Sica explained that the government chose them "*because they make up a large share of the basic food basket consumed by the population*" (Infobae, August 15, 2019). The Secretary of Consumer Protection, Fernando Blanco Muiño, separately explained that fresh and seasonal items such as meats, fruits, and vegetables were not included because they are more difficult to monitor (La Gaceta, August 15, 2019). In none of these communications do officials mention selecting goods based on their exposure to the exchange rate shock; the consistent rationale is that the targeted goods make up a large share of food expenditure for Argentine households.

Importantly, the price data we exploit in this paper originates from a different source (it is collected from supermarkets by a private firm, as we explain in Section 3.1).

2.3 Anti-Profiteering Measures

The new Fernandez administration did not extend the VAT holiday and instead regulated the reintroduction of the 21% VAT rate on a large share of the previously zero-rated goods. To limit the impact on consumer prices, the government implemented anti-profiteering measures that capped how much prices could increase upon the reinstatement of the VAT. In this section, we describe these anti-profiteering measures, how they were implemented, and situate them within the broader international experience.

Legal Framework. The anti-profiteering measures were implemented under two preexisting laws. The first is the "*Ley de Abastecimiento de Comercio Interior*" (Law 20,680/1974), which grants the government broad powers to intervene in markets. The second is a 2013 law that introduced the "*Precios Cuidados*" program, allowing the government to set price controls on grocery items. Together, these laws provided the legal basis to cap price increases in supermarket chains following the repeal of the VAT cut. The government determined and announced the caps on December 31, 2019, communicating them to supermarkets and the public via press releases.

While the prices of some of the goods that were treated by the VAT cut and its repeal could change freely, others were only allowed to increase by up to 7% or 10.5% of their December 31, 2019 price once the VAT cut was repealed. Still others were mandated to remain unchanged (e.g., liquid milk). Table 2 summarizes these caps by product category. These caps were legally enforced only at supermarket chains, which meant that independent stores could adjust their prices freely.

Enforcement. Enforcement fell under the jurisdiction of the agencies responsible for consumer protection and antitrust regulation. Chain supermarkets were monitored daily through the *"Precios Claros"* system described above. This infrastructure was also used to track compliance with the anti-profiteering measures following the VAT increase. In interviews with senior officials (including Matías Kulfas, then Minister of Productive Development), we confirmed that they received daily price reports showing how much prices changed from day to day. Kulfas, who chaired the December 31, 2019 meeting with food manufacturers and supermarket associations, noted that violations could trigger investigations and sanctions by the Commerce Department under the newly enacted Fair Commerce Law (Decree 274/2019). While rare, exceptions to the caps were granted when firms could justify price increases due to higher input costs.[6]

This new Fair Commerce Law empowered the government to impose a wide range of penalties

[6] Appendix Figure E.3 shows an excerpt from *Clarin*, one of Argentina's leading newspapers, discussing enforcement of the anti-profiteering measures. The image features Matías Kulfas, who led the implementation of these measures.

for non-compliance, including formal warnings, monetary fines ranging from 1 to 10 million Mobile Units (each initially valued at 20 pesos in 2018 and indexed to inflation), suspensions from the National Register of Government Suppliers, revocation of concessions, and even temporary business closures for up to 30 days. Several firms were sanctioned under this framework, as documented by contemporaneous media reports.[7]

We provide empirical evidence on compliance with these measures and the challenges with enforcing it in more detail in Section 4, where we show that these caps were binding, but that there was some non-compliance.

Caps Horizon. The duration of the anti-profiteering measures was never formally specified, leaving supermarkets uncertain about how long they were expected to comply with the price caps. In practice, the Ministry of Productive Development and the Commerce Department monitored and investigated price changes on a daily basis from January 1, 2020 until the onset of the COVID-19 pandemic in March 2020, when enforcement activities were effectively suspended.

Anti-Profiteering Measures in Other Countries. Appendix Section A provides a non-exhaustive overview of anti-profiteering policies implemented across other developing countries. It might come as a surprise that some features of the anti-profiteering measures were vague, such as the time horizon over which they were expected to apply. However, anti-profiteering measures in other countries are also often imprecise. This is the case with India's enactment of anti-profiteering measures in 2017, which targeted consumption tax cuts and credits. Similar to the Argentine experience, the legal framework of the Indian anti-profiteering measures is relatively vague in that it does not define profiteering explicitly and leaves it to the Competition Commission of India (an institution akin to the U.S. Federal Trade Commission) to decide on a case-by-case basis (see Kir, 2023).[8]

These anti-profiteering policies are also relatively unclear when implemented in OECD countries. For example, France mandated the incidence of a large VAT cut on the sit-down restaurant industry in 2009 by requiring restaurants to pass through one-third of the VAT cut to prices, one-third to profits, and one-third to wages, but did not specify how long these mandates should remain in effect or how they would be enforced. Similarly, California recently passed Bill SBX1-2 which allows the government to monitor the profit margins of oil companies and sue them if they are considered to be too high, but does not specify what is considered to be too high.[9]

[7] See, for example, La Nación, El Diario Córdoba, and Gobierno de Río Negro.

[8] Section 171 of the CGST Act is remarkable in that it states: "Any reduction in rate of tax on any supply of goods or services or the benefit of input tax credit shall be passed on to the recipient by way of commensurate reduction in prices." Any consumer or organization that deems the price reduction insufficient can file a claim against the party at fault, which is then investigated by the Competition Commission of India, as described on their website.

[9] See link.

2.4 Salience

Both the VAT cut and the subsequent VAT increase were highly publicized in the media and in supermarkets, suggesting they were highly salient. For example, Appendix Figure E.4a shows the front page of the two main newspapers in Argentina, one day after the VAT holiday was announced. In both cases, the front-page articles are about the VAT cut. Similarly, Appendix Figure E.4b shows the front page of the same newspapers one day after the VAT cut was repealed. The main articles cover the VAT change and how price increases were regulated under different caps. Finally, Appendix Figure E.5 shows how supermarkets communicated the VAT cut to their customers using fliers and price tags, which were mandated by the government. It is unclear whether supermarkets expected the government to impose price caps during the VAT increase. On the one hand, these anti-profiteering measures were announced on December 31, a day before they were enacted. On the other hand, Argentina has implemented several price caps and freezes in the past, and so such a policy may not have been a surprise to supermarkets.

3 Data and Empirical Strategy

3.1 Data

We provide a summary of all the datasets we use in Appendix Table A2 and describe them in detail below.

Supermarket Scanner Data. Our analysis primarily relies on retail scanner data provided by the marketing consulting firm Scentia LLC.

These data consist of high-frequency sales records generated by point-of-sale systems across Argentina. In particular, Scentia gathers all scanner-based price and quantity information from both chain and independent supermarkets (it also collects data from wholesalers, pharmacies, and convenience stores at gas stations, which we did not purchase). Among supermarket chains, the sample covers the 12 largest retail chains in the country, encompassing all 2,317 stores (e.g., Walmart, Carrefour, Coto, La Anonima, etc.). For independent supermarkets, Scentia collects data from a representative sample of 800 stores, drawn from a universe of roughly 18,700 establishments nationwide. These stores consist mostly of independent supermarkets and a few regional chains owned locally (such as Buenos Días, Cordiez, and El Nene) rather than "mom and pop" shops or convenience stores. Because these data are derived from barcode scanners used for inventory and stock management, they capture all scanned items regardless of payment method (cash or card) or whether a receipt is issued. As such, scanner-based systems ensure comprehensive transaction coverage as long as the product is scanned at checkout.

Scentia's database contains the following variables: time period, EAN barcode, unit price paid at the cash register (including taxes and discounts), purchased quantities, total volume, a detailed label describing the item, the brand, the producer, and the region. All products are categorized into broad categories (e.g., oil, coffee, rice), which are further subdivided into subcategories (e.g., sunflower oil, corn oil, olive oil, ground coffee, coffee beans, coffee pods). In addition, each product carries a rich textual description (e.g., Nescafé Gold Intense Instant Coffee Jar 200g), which provides granularity below the subcategory classification. This detailed labeling allows us to accurately assign products to treatment and control groups, as shown in Tables 1 and 2.

We use three different data extracts from Scentia. The first and primary data extract used in the analysis is aggregated across store types at the barcode-region-time level. We refer to this data extract as the "main sample". For chains, we observe weekly barcode data from ten geographic areas. For independent supermarkets, we observe monthly information from barcodes split into five regions.[10] Our dataset covers January 2018 through June 2021 (181 weeks for chains and 42 months for independent supermarkets). When aggregated at the region-by-barcode-by-month level, each month covers an average of USD 170 million worth of grocery sales across 3,117 individual stores, in more than 60 disaggregated product categories, and across 19,304 barcodes, which correspond to 642 producers and 1,248 brands. This extract only covers food products. Within each barcode–region–store-type-time cell, Scentia constructs the price as a unit value (total expenditure divided by total quantity sold across the stores in that cell), so the within-cell aggregation is already quantity-weighted by sales. Chain-store coverage is a census (all 2,317 stores in the 12 largest chains), while independent-store coverage is a representative sample (800 stores drawn from a universe of approximately 18,700 independent supermarkets nationwide).

In our analysis, we use a balanced panel of the main sample that includes all barcode–region–store-type cells with positive sales in the 15-month period from January 2019 to March 2020. Appendix Table A3 provides summary statistics for this panel across chain and independent supermarkets, disaggregated by whether they were subject to the VAT cut and its repeal, and whether their prices were capped by the anti-profiteering measures. The table also presents baseline average price differences. The two samples are broadly comparable but differ in two respects: (1) there is a significant portion of barcodes that are only sold in one type of store; and, (2) average prices in independent supermarkets, at baseline (i.e., prior to the VAT cut), tend to be 3 to 4% lower than in chain supermarkets, which could reflect compositional differences (see point (1)) or intrinsic differences in prices levels.

We use two additional Scentia extracts for robustness checks. The first — the "Periferia sample" — mirrors the main sample in structure but is restricted to the Periferia region and chain supermarkets, observed at weekly frequency, and expands coverage to beverages and non-food items (barcode–*Periferia*–week cells). The second is more disaggregated, containing monthly store-level observations for both chains and independent supermarkets in the Córdoba region (barcode–*Córdoba*–store-id–month cells), covering food and non-food items and including quantities as well

[10]The ten regions are: Capital Federal, Periferia, Córdoba, Litoral Norte, Litoral Sur, Resto Pcia BSAS, Cuyo, NOA, Sur, Austral. The five broader areas are: Andina, Córdoba, GBA, Litoral, Resto Pcia BSAS + Sur. The data is representative of the whole country. See Appendix Figure E.6 for more details about geographic variables.

as prices, with precise geographic information for chains.

VAT return data. This source contains semi-aggregated information derived from firm-level VAT return data. We use it to estimate the effect of the VAT reform on government revenue. It is produced by the National Tax Agency and covers all regions of Argentina annually from 2015 to 2023. It is aggregated at the sectoral level.

National Household Expenditure Survey. In addition to the datasets described above, we use detailed expenditure survey microdata from the 2017-2018 National Household Expenditure Survey (ENGHo), conducted by the National Institute of Statistics and Censuses (INDEC). The ENGHo provides detailed product-level information on both food and non-food expenditures, including the types of stores where purchases were made, the payment methods used, and a range of household characteristics. The data were collected through a questionnaire answered by the head of the household and weekly diaries used to record daily household expenditures. The survey was conducted between November 2017 and November 2018 in localities with 2,000 or more inhabitants, yielding a nationally representative sample of approximately 45,000 households.

Consumer Price Index. This dataset, produced by INDEC, contains the product-level prices used to construct Argentina's Consumer Price Index (CPI). We use it to validate the representativeness of the Scentia scanner data in capturing national price dynamics.

3.2 Empirical Strategy

Our empirical analysis relies on a dynamic difference-in-differences approach. We classify products into treatment and control groups depending on whether a given barcode was subject to the policy intervention under analysis (VAT cut, VAT increase with price caps, etc.). Our empirical specification is given by:

$$Y_{i,r,s,t} = \alpha_{i,r,s} + \gamma_t + \sum_{t \neq t^*} \beta_t D_{i,r,s,t} + \epsilon_{i,r,s,t} \tag{1}$$

where the unit of observation is a barcode i in region r and store type s (chain or independent), observed at time t (weeks or months). $Y_{i,r,s,t}$ denotes the tax-inclusive price for the cell, which Scentia constructs as a unit value across the stores in the cell as described in Section 3.1. We normalize $Y_{i,r,s,t}$ to 100 at the reference period t^* (2019w32 or July 2019, depending on whether the data are weekly or monthly); $\alpha_{i,r,s}$ are barcode–by–region-by–store-type fixed effects, γ_t are time fixed effects, and $D_{i,r,s,t}$ equals one if cell (i, r, s) is treated at time t and zero otherwise. Equivalently, $D_{i,r,s,t}$ is the interaction between a time-invariant treatment dummy $D_{i,r,s}$ (equal to one for cells in the treatment group) and an indicator for period t; the summation over t therefore produces event-study coefficients β_t that test for parallel pre-trends in the pre-policy periods and report the dynamic treatment effects from the cut onward. The coefficients of interest β_t capture the dynamic treatment effects — the average price difference between treated and control cells in each period, relative to the baseline t^*. When we pool chains and independents, the regression operates on cells from both store types; when we estimate the specification on chains or on independents alone, we drop the corresponding cells. Standard errors $\epsilon_{i,r,s,t}$ are clustered at the product–region level—the level at which treatment is assigned (Abadie et al., 2023), where products are the treated and untreated categories in Table 1 and regions are the broader geographic units defined in Appendix Figure E.6—and, because this leaves a moderate number of clusters, we base inference on the wild cluster bootstrap (Cameron et al., 2008).[11]

The treatment and control groups include all barcodes belonging to the food categories described in Section 3.1 and listed in Table 1. Although the VAT rate on wheat flour was reduced to zero, we exclude these barcodes from the analysis because flour was already subject to a lower baseline VAT rate of 10.5%, rather than the standard 21%. We also exclude barcodes covered by the *Precios Cuidados* and *Productos Esenciales* price-agreement programs (Section 2.3), under which the government negotiates and administers the prices of a subset of grocery items.[12] Because the prices of these goods are set under separate arrangements rather than freely by the market, they would not respond to the VAT change in the same way, and retaining them in the treatment or control group would blend administered prices into our pass-through estimates. We apply this exclusion symmetrically to the treatment and control groups. The control group comprises all barcodes in the following categories: Other cooking oils (olive, soy, canola); rice-based meals; Breakfast cereal; Coffee; Salt; Herbs, Spices, & Seasonings; Dulce de leche; Jam and Jelly; Other flours; Crackers and Biscuits; Chocolate; Mayonnaise; Vinegar; Dried legumes and beans.

4 Results

We first estimate the effects of the VAT cut and its subsequent repeal on the prices of goods that were not subject to anti-profiteering measures. We then turn to goods that were covered by such measures and estimate the corresponding price effects. Finally, we disaggregate the analysis by store type—chain supermarkets, where anti-profiteering measures were enforced, and independent stores, where such measures did not apply.

4.1 VAT Cut and Increase *Without* Anti-Profiteering Measures

Figure 1a presents the nonparametric effect of the VAT cut and its repeal on prices in the control and treatment groups. Here, the treatment group includes only barcodes that were not subject

[11] Because the wild cluster bootstrap is computationally infeasible on the largest weekly chain samples (several million barcode-week cells exceed available memory), we estimate the banded difference-in-differences figures at the monthly frequency, where the bootstrap is feasible; aggregating to monthly leaves the estimates essentially unchanged (Appendix Figure E.7). Two figures are estimated at the weekly frequency: the chain pass-through panel (Appendix Figure E.7, Panel (a)), for which we report the analytic product–region cluster-robust interval rather than the bootstrap, and the smaller *Periferia* non-food subsample (see Section 4.4), for which the bootstrap remains feasible.

[12] We study the equilibrium effects of these price-control programs in a companion paper by Barahona, Benzarti, Díaz de Astarloa, García-Lembergman, Garriga, Jiménez-Hernández, and Tortarolo (in progress).

to anti-profiteering measures and could therefore adjust prices freely upon the repeal of the VAT cut. The dataset used in this figure pools chain and independent supermarkets together, thus the observations are at the monthly level.

Four findings are worth highlighting. First, prices in the treatment and control groups follow parallel trends in the six months preceding the VAT cut. Second, there is a sharp break in the series immediately after the VAT cut was implemented, as prices in the treatment group grow at a substantially lower rate than those in the control group. Note that prices trend positively, since we are plotting nominal prices and inflation was high ($\approx 55\%$ yearly in 2019). Third, another break in the series occurs in January 2020, when the VAT cut is repealed. Here, prices in the treated group rise sharply—surpassing those in the control group and exceeding their pre-VAT-cut counterfactual levels. Fourth, prices in the treatment group do not appear to be converging down to those of the control group, suggesting that the asymmetric response of prices to the VAT cut and increase might be long-lasting. Note that the figure ends in March 2020, the end of our main 15-month analysis window; the longer-horizon figures extend beyond it to document the persistence of these effects.

Figure 1c (the no-caps series) plots the results of estimating equation (1) on the exact same data as in Figure 1a, which allows us to add standard errors and precisely estimate the magnitude of the effect of the VAT cut on prices. Overall, the results we get from estimating (1) closely match those of the unconditional means plotted in Figure 1a. First, we find that the trends are mostly parallel with a small difference between treatment and control groups in the pre-reform period. This issue disappears once we consider a larger sample below. We also find that prices decrease on average by 9.3 percentage points over the four-month period following the VAT cut, excluding the August point estimate, which is mechanically partially treated since the VAT cut was passed on August 16th. This corresponds to a pass-through rate of the VAT cut to prices of 53% relative to the full pass-through benchmark of 17.4 percentage points (a full pass-through would yield a price decline of $-0.21/1.21 \times 100 = 17.4\%$, since the VAT rate fell from 21% to 0%).

Finally, our estimates confirm that prices respond to the repeal of the VAT cut in the treatment group and exceed their pre-VAT cut levels by 6.0 percentage points, with no evidence of convergence to zero. Table 3 provides a compact reference summary of our main pass-through estimates by store type (all stores, chains, and independent supermarkets) and cap status (all treated goods, uncapped, and capped) at the monthly frequency; we refer back to it throughout the remainder of this section. Overall, the pooled estimate falls short of full pass-through, and the VAT increase leads to prices that are higher than their pre-VAT cut levels, with some evidence of hysteresis. As we show below, however, this pooled pattern masks substantial heterogeneity by store type: in chain supermarkets, where the government monitored prices, pass-through of the VAT cut is near-complete once we correct for substitution and the peso depreciation (Section 4.4).

4.2 VAT Cut and Increase *With* Anti-Profiteering Measures

While there was no formal government regulation of how much of the VAT cut supermarkets should pass through, several anti-profiteering measures were put in place for the VAT increase (see Table 2); we refer to these price controls as *caps on how much prices could increase* to mitigate the VAT reintroduction. In particular, regular rice (long grain white), dried pasta, tea, yerba mate, mate cocido, sugar, canned vegetables and beans, corn and wheat flour, and regular yogurt were subject to a 7% cap on price increases. Furthermore, milk was subject to a 0% cap, i.e., its price was held nominally fixed. On the other hand, corn oil, other rice (basmati, brown, and organic), canned fruits, and yogurts with fruits or cereals mixed in, were not subject to any price controls. These price controls were legally enforced only in chain supermarkets, and not in independent supermarkets; this was mostly because the government had limited capacity to enforce the regulation and monitor prices across the more than 18,000 independent stores nationwide.

This setting offers a unique opportunity to estimate the effect that governments can have on tax incidence. In order to assess the effect of these anti-profiteering measures, we break down the list of goods into a control group (unaffected by the VAT cut) and a treatment group, which corresponds to those barcodes that were treated by the VAT cut and that were also subject to the price caps when the VAT was increased.

Chain and independent supermarkets pooled together. Figure 1b shows the nonparametric effect of the VAT cut and its repeal on prices, in the control and treatment groups (the latter includes only those goods that were subject to the price caps). In this figure, we pool chain and independent supermarkets together. There are three main findings. First, the trends for the control and treatment groups are parallel. Here, since the sample of goods we consider is substantially larger than that for uncapped barcodes, the parallel trend assumption holds even better than above. Second, there is a sharp break in the series immediately after the VAT cut. Third, there is another break in the series when the VAT cut is repealed. Here, prices in the treatment group increase enough to match those in the control group, thus restoring the previous equilibrium (with no asymmetry). Our difference-in-differences estimates confirm these patterns (Figure 1c and Table 3).

Pooling chain and independent supermarkets together allows us to assess the overall effect of the VAT cut along with the anti-profiteering measures, even though independent supermarkets were not subject to them. Overall, the anti-profiteering measures compressed the asymmetric pass-through even at the aggregate level. We estimate the consumer surplus effect of the anti-profiteering measures formally in Section 5.

Chain and independent supermarkets separately. Figure 2a plots the difference-in-differences estimates of the temporary VAT cut and its repeal, separately for chain and independent supermarkets. The treated group in panel (a) pools goods that were later subject to the anti-profiteering caps in chains and those that were not. Importantly, the cap distinction binds only in chain supermarkets (Section 2.3).

We find markedly different price effects of the VAT cut and its repeal in chain and independent supermarkets. The pass-through rate of the VAT cut is 84% for supermarket chains and 35% for independent supermarkets. The 84% for chains is conservative: once we correct for substitution across goods and the concurrent peso depreciation, chain pass-through rises to near-complete—within roughly a percentage point of full pass-through (Section 4.4).[13] The two store types differ just as sharply on the VAT *increase*. In chain supermarkets, where the anti-profiteering caps bound, prices rose well short of full pass-through and capped goods settled *below* their pre-cut baseline, whereas in independent supermarkets the increase passed through far more, leaving prices persistently above it (Table 3). It is this large gap between the two store types in the pass-through of the VAT *increase* that we trace to the anti-profiteering measures below.

The bottom event-time panels of Figure 2 make both the asymmetry and the resulting hysteresis legible, provided the two normalizations are kept distinct. Panels (b) and (c) plot each reform against its own pre-reform baseline month and against its full-pass-through benchmark—the horizontal dashed lines at -17.4 and $+21$ percentage points. These benchmarks differ because a 21% VAT rate is 21% of the net price but only 17.4% of the gross, tax-inclusive price.[14] A full round trip would therefore return prices exactly to baseline, so any deviation after it reflects entirely the *difference* in pass-through between the two reforms.

In independent supermarkets, pass-through is incomplete in both directions but substantially higher for the repeal than for the cut—an asymmetry large enough to leave prices persistently above their pre-reform level. For instance, for uncapped goods (Panel 2b), the VAT cut is passed through at about 35% (-6.1 of a possible -17.4 percentage points) and the increase at about 64% ($+13.5$ of a possible $+21$; Table 4). Chaining the two leaves prices about 7% above where they began.[15] Averaged across all treated goods, independent-store prices settle roughly 5% above their pre-cut level—the hysteresis that Panel 2a displays directly.

The pattern of incomplete and asymmetric pass-through that we estimate for independent supermarkets is consistent with the prior VAT-incidence literature. In particular, Benzarti et al. (2020) document that pass-through of VAT increases is substantially larger than pass-through of VAT cuts in less-monitored settings: roughly twice as large in their Finnish hairdresser case study, and roughly three times as large in their EU-wide panel of VAT reforms. Our independent-store estimates fall within this pattern. For uncapped treated goods in independent supermarkets, the

[13] Appendix Figure E.7 shows that aggregating the weekly chain data to monthly barely affects the estimates: 14.7 percentage points at the weekly level versus 14.9 at the monthly level.

[14] Removing the tax takes the gross price from $1.21\times$net down to net, a fall of $(1-1.21)/1.21 = -17.4\%$; reinstating it raises the price from net back to $1.21\times$net, a rise of $(1.21-1)/1 = +21\%$. Under full pass-through in both directions the two exactly offset, returning the price to its starting value ($0.826 \times 1.21 = 1$).

[15] From a pre-cut index of 100, the cut lowers the price to $100 \times (1 - 0.35 \times 0.174) \approx 94$, and the increase raises it to $94 \times (1 + 0.64 \times 0.21) \approx 107$.

pass-through of the VAT cut is approximately 35%, and the pass-through of its re-introduction is approximately twice as large over the months following the repeal (Figure 2b). One plausible driver in less-monitored settings like ours—and one that is especially relevant for other developing countries—is partial VAT evasion, which dampens the pass-through of both VAT cuts and increases. It cannot be the whole story, however, since the same asymmetry appears in the more formal, low-evasion settings that Benzarti et al. (2020) study. We discuss this and related mechanisms in Appendix B.3.

Comparing capped and uncapped goods by store type. We now examine the anti-profiteering caps directly: how much they compressed the price increase on capped relative to uncapped chain goods, how compliance differed between percentage caps and outright price freezes, how persistent the cap-induced gap proved, and how the four store-type-by-cap-status cells compare.

The large difference in the pass-through rate of the VAT increase between chain and independent supermarkets is likely due to the anti-profiteering measures, which were in place only in chain supermarkets. To confirm this, Figure 3a compares the change in prices for those commodities that are subject to the 7% price increase cap with those with no price caps, in chain supermarkets. To make the persistence of the cap-induced gap visible, this figure uses an extended estimation window that runs through the end of 2020, beyond the main analysis window used in earlier figures. In both cases, the control group is the original set of barcodes subject to the 21% VAT rate. This figure shows that goods with no price caps experience a price increase (when the 21% rate is reintroduced) that almost doubles that of those subject to the 7% cap (Appendix Figure E.8 corroborates this with two case studies—regular versus other rice, and canned fruit versus canned vegetables). A concern with using uncapped treated goods in chains as a counterfactual for capped treated goods is that, under monitoring, chains may have shifted profit-making across goods. In Section 4.4 we test this directly using cross-store variation in pre-reform exposure to the caps: the stores most exposed to the caps do not raise uncapped-goods prices more than less-exposed stores—the opposite of what such profit-shifting would predict—so we find no evidence for this mechanism.

However, while the 7% cap reduces average price increases relative to uncapped chain goods, Figure 3a shows that some barcodes exhibit price increases of more than 7%. This could be due to the fact that monitoring percentage increases in prices can be difficult in a high-inflation environment. This is confirmed in Figure 3b, which shows that when price controls take the form of a price freeze, i.e., holding the nominal price fixed, as was the case for milk, there is more compliance, since average prices experience a limited increase at the time of the VAT increase.

We also show that the anti-profiteering measures had long-lasting effects, even after the cap enforcement was suspended at the onset of the COVID-19 pandemic. Figure 3a shows that the price gap between products with and without price caps remains stable until the end of 2020.

Finally, we compare chains and independent supermarkets separately for uncapped goods and for goods in the capped categories (Figures 2b and 2c). Table 4 is the most direct numerical summary of this asymmetry: it reports the regression counterpart of these two panels, estimating each reform as a separate difference-in-differences measured against its own pre-reform month, for all four (store type) × (cap status) cells. The incomplete and asymmetric pass-through of independent supermarkets is present for both groups: independents pass through only part of the VAT cut and end above their pre-cut level whether or not the goods fall in the capped categories. In both store types the repeal increase is smaller for capped-category goods than for uncapped goods, but the gap is larger in chains—where the caps directly bind (+11.4 versus +18.0 percentage points averaged over the five months following the increase; Table 4)—than in independent supermarkets (+10.0 versus +13.5 percentage points), which were never subject to the caps.

Taken together, the chain-cap results above also highlight a pitfall of price-cap policies that has not always been emphasized in the literature: in the absence of information on marginal cost, an ex-ante cap risks generating an asymmetric distortion in the opposite direction when the underlying price response is itself asymmetric. We see direct evidence of this in our setting — capped goods in chains settled persistently below their pre-VAT-cut levels relative to control (Table 3), reflecting binding caps interacting with the asymmetric pass-through documented elsewhere in the paper.

Distribution of Price Changes. Using the weekly frequency of chain supermarket data, we examine the distribution of price changes around the VAT increase to assess the extent to which the anti-profiteering measures were binding. Figure 4 shows the distribution of price changes for three groups: control goods unaffected by the VAT changes; treated goods subject to the 7% anti-profiteering cap on how much prices can rise; and treated goods not subject to any cap. For each group, we measure the percentage-point price change relative to one week before the VAT increase, without differencing out the control group. Milk, subject to a 0% cap (i.e., prices were mandated to remain unchanged), is shown separately in Appendix Figures E.9 and E.10.

The distributions for weeks 0 to 3 following the VAT increase reveal two clear patterns. First, the three groups are visibly distinct from week 1 onward. Second, there is pronounced bunching at the relevant policy thresholds: at 0% for control goods, at 7% for capped goods, and at 21% for uncapped treated goods—consistent with near-complete pass-through to consumers in the absence of caps. The bunching at 7% confirms that the anti-profiteering measures were binding for many goods, though the non-negligible mass above 7% indicates partial non-compliance. These patterns persist through weeks 4 to 11 (Appendix Figure E.11), becoming more diffuse over time but remaining clearly distinct across groups—consistent with the sustained price gap documented in Figure 3a.

Taken together, these distributions confirm that the anti-profiteering measures compressed price increases for capped goods, while partial non-compliance explains why average capped prices remained above what full enforcement would have implied.

Asymmetry and Hysteresis. Our results document three puzzling facts. Canonical tax incidence models predict symmetric pass-through of equal-sized tax changes (see Fullerton & Metcalf, 2002; Benzarti, 2025). The usual explanations for departures from symmetry rely on price rigidities, but rigidities should be less binding in the high-inflation environment we study, which makes the asymmetry we find genuinely puzzling. The first puzzle is that the prices of uncapped goods treated by the VAT cut exhibit asymmetric pass-through to the VAT decrease and to its repeal: the pass-through of the VAT increase is substantially higher than that of the VAT cut. We argue in Appendix B that this asymmetry can be rationalized within menu-cost macro pricing models that allow firms to bundle inflation-induced price changes with the VAT-induced change. The second puzzle is that the prices of uncapped goods exhibit hysteresis: they do not converge to pre-VAT-cut levels several periods after the repeal of the VAT cut. The third puzzle is that the cap-induced gap between capped and uncapped chain goods — approximately 9.5 percentage points at the VAT repeal in January 2020 (Table 3) — persists essentially unchanged through the end of our sample in December 2020, nine months after enforcement of the caps was effectively suspended in March 2020 with the onset of the COVID-19 pandemic (Figure 3a). This persistence is particularly puzzling because the caps were binding while in force — the distribution of price changes for capped goods bunches sharply at the 7% threshold (Figure 4) — and because the firms whose capped-goods prices fail to converge upward once enforcement ceases are demonstrably capable of rapid price adjustment, as their responses to both VAT changes show. The second and third puzzles are related in that they both show that prices exhibit hysteresis when responding to taxes or price regulations — a finding documented in other settings as well (Benzarti et al., 2020), and one that standard macro pricing models cannot explain, as we discuss in Appendix B.

4.3 Quantity Responses

Our scanner data records quantities sold alongside prices, allowing us to estimate demand responses to VAT changes. Doing so requires additional care, because food quantities are strongly seasonal and the seasonal profile differs across goods: coffee, a control good, peaks in the winter months and troughs in the summer, while many treated goods peak at other times of the year (Appendix Figure E.12). A difference-in-differences in the level of log quantities would load this differential seasonality onto the estimated treatment effect. We therefore measure the quantity response with a year-over-year outcome, $\Delta^{12} \log Q_{i,r,s,t} = \log Q_{i,r,s,t} - \log Q_{i,r,s,t-12}$, which compares each barcode i in region r at store type s to the same calendar month a year earlier, thereby differencing out any fixed annual seasonal pattern, as is standard when working with high-frequency data (Chetty et al., 2024). Appendix Figure E.13 reports the resulting dynamic effects from estimating equation (1), separately for chain and independent supermarkets.

The quantity response mirrors the price results. Pre-reform coefficients are flat, consistent with parallel trends in year-over-year growth. Following the VAT cut, quantities of treated goods rise by roughly 11 to 16 percent in chain supermarkets, concentrated in the months immediately after the cut, while the response in independent supermarkets is small and statistically insignificant—the same chain-independent gap we find for prices, and consistent with the larger pass-through and greater salience of the cut in chains. The implied own-price elasticity of demand, the ratio of the quantity response to the estimated price change, lies between -0.7 and -1.0, in line with elasticities estimated from grocery scanner data in other settings (Allcott et al., 2019; DellaVigna & Gentzkow, 2019).

Because the policy was meant to preserve access to basic food as the peso depreciation raised prices, rather than to stimulate consumption, the increase in purchased quantities—concentrated in the chains where higher-income households tend to shop—is plausibly an unintended consequence of the reform. Two features of the series should be read as artifacts rather than treatment effects: a dip around the January 2020 re-introduction, as prices and demand revert, and a sharp rise from March 2020, when COVID-19 stockpiling of staples (over-represented among treated goods) comes to dominate the series. The store-level Córdoba analysis in Section 4.4 (Income Effects), which holds the barcode and region fixed and is therefore robust to seasonality by construction, provides complementary evidence on quantity responses.

4.4 Robustness Checks

This subsection shows that our main findings — near-complete pass-through of the VAT cut in chain supermarkets, asymmetric and incomplete pass-through in independent supermarkets, and a persistent gap between the two — are robust across a range of exercises. These include accounting for substitution and the peso depreciation (Figure 5), relaxing the balanced-panel restriction (Appendix Figure E.21), winsorizing price changes (Appendix Figure E.24), alternative weighting and aggregation schemes (Appendix Figures E.25a, E.25b, and E.26, and Appendix Table A4), and a test for profit-shifting across capped and uncapped goods using cross-store variation in cap exposure (Appendix Figure E.27).

Substitution across products in the treatment and control groups. A standard concern with difference-in-differences designs in product markets is that consumers may substitute between treated and control goods, biasing treatment effect estimates. For example, if the VAT cut lowers the price of tea, some consumers may shift from coffee to tea, increasing demand for treated goods and pushing their prices up, thereby biasing our effects downward (e.g., see Minton & Mulligan, 2024).

We address this potential violation of the Stable Unit Treatment Value Assumption (SUTVA) using two strategies. First, we redefine the control group by excluding categories likely to be close substitutes for treated goods — rice-based meals, coffee, cooking oils, dried legumes, other flours, soups, and prepared pasta — retaining items such as breakfast cereal, salt, herbs, and jams that have no obvious substitutes among treated products (Table 1). Re-estimating the dynamic difference-in-differences using this restricted control group leaves the estimates essentially unchanged in both store types (Appendix Figure E.14): for chain supermarkets (Panel (a)) the average price decrease after the VAT cut is 15.2 percentage points, compared to 14.7 in the baseline, and for independent supermarkets (Panel (b)) it is 6.7 percentage points, compared to 6.2 in the baseline. Substitution operates in the expected direction but has minimal effect on the estimates in either store type.[16]

Second, we re-estimate our main effects using an alternative control group composed exclusively of non-food items, which are highly unlikely to be substitutes for food products. These non-food products, previously excluded from our specification, include office supplies, body moisturizers, antiperspirants, hand soap, laundry detergent, bleach, surface cleaners, toilet paper, shampoo, and cleaning wipes. For this analysis, we use the Periferia sample, which is restricted to chain supermarkets in a single region (*Periferia*), as we were only able to purchase non-food scanner data for that region. This restriction results in larger standard errors. The average price of treated goods fell by 16.9 percentage points relative to the non-food control group—larger than the baseline estimate and close to the full pass-through benchmark, consistent with the baseline being modestly biased downward by substitution into the food control goods; we report this alongside the other chain-supermarket corrections in Figure 5, discussed below. Taken together, the two strategies indicate that substitution operates in the expected direction and biases the baseline modestly downward; correcting for it—most cleanly with the non-food control—moves chain pass-through close to the full pass-through benchmark.

Pass-through of the peso depreciation. The Argentine peso depreciated sharply three days before the VAT cut, posing a potential threat to identification. If this depreciation affects basic necessities subject to the VAT cut more than the control group, then we would expect an upward pressure on those prices. The pass-through rate of the VAT cut to prices would be partially offset, yielding a more conservative estimate. In other words, absent the depreciation of the peso, the prices of the treated goods would have decreased even more.[17]

To address this concern, we use another depreciation episode that took place exactly one year before the VAT change and compare the prices of treated and control products. On August 30, 2018, Argentina experienced the second-largest depreciation of the peso since 2002—similar in magnitude to the depreciation episode of August 12, 2019 (Appendix Figure E.1): between January and September of 2018, the exchange rate doubled and remained stable thereafter. We run our baseline specification (1) for supermarket chains in 2018 and 2019, up to the week before the VAT cut, omitting the first week of 2018 so that all coefficients are measured relative to it.

[16] The left panel of Appendix Figure E.15 provides more details on the substitution mechanism by comparing price changes in tea and different types of coffee relative to the remaining control categories. While instant coffee exhibits a price decrease, ground coffee does not. The right panel shows that breakfast cereal prices do not respond, while sliced bread prices fall sharply during the VAT holiday.

[17] In principle, imported goods are more likely to be affected by the depreciation of the Argentine peso. Nonetheless, we find similar pass-through rates when we exclude imported goods from the treatment and control groups (Appendix Figure E.16), suggesting that the currency depreciation episode did not affect these groups differently.

Appendix Figure E.17a shows that the prices of the treated goods indeed responded more to the depreciation of the peso back in 2018. The price gap between the treatment and control groups closely tracks the evolution of the exchange rate. Relative prices remain stable up to week 25 of 2018, then start to increase *pari passu* with the exchange rate and stabilize again after week 45. This evidence suggests that the government may have successfully targeted necessities likely to be affected by the 2019 peso depreciation to alleviate the burden on low-income households.[18]

Nevertheless, the magnitude of this depreciation effect is small and poses no significant threat to identification. According to Appendix Figure E.17a, the nominal exchange rate roughly increased from 20 to 40 pesos per dollar—corresponding to a 100% increase. In contrast, the prices of the treated goods increased by 6% relative to the control group. By scaling the price change by the exchange rate change, we obtain an elasticity of 0.06. By applying this elasticity to the depreciation of the peso of 24% in 2019 (Appendix Figure E.1), we conclude that, absent the VAT cut, prices of treated goods would have increased by $0.06 \times 0.24 = 1.44\%$ relative to the control group. This means that, absent the depreciation of the peso, the price drop reported in Figure E.7 would be 1.44 percentage points larger.

Combined corrections: chain pass-through. Combining the substitution and peso-depreciation corrections, the chain-supermarket pass-through of the VAT cut moves to within roughly a percentage point of the full pass-through benchmark of 17.4 percentage points. Figure 5 shows this graphically for the *Periferia* subsample, where non-food data are available for a clean substitution check: the baseline series and the three corrected series (non-food control group, close-substitutes-excluded control group, and depreciation-adjusted baseline) all sit on or near the full pass-through line during the VAT-cut period. The corrected estimates thus confirm near-complete pass-through in chains, in contrast with previous estimates from less-monitored settings. In this context, the asymmetry that motivates the anti-profiteering measures is not incomplete pass-through on the cut but the response to its repeal: with the cut close to fully passed through, the policy-relevant concern shifts to the repeal, where—in the less-monitored independent stores—incomplete and asymmetric pass-through leaves prices above their pre-cut level (Figure 2a).

Pass-through in independent supermarkets after corrections. The analogous substitution and peso-depreciation corrections applied to independent supermarkets leave the pass-through estimate well below the full pass-through benchmark. Appendix Figure E.19 shows that the baseline, close-substitutes-excluded, and depreciation-adjusted series for independent stores all sit between roughly -7 and -9 percentage points during the VAT-cut period — well below the -17.4 p.p. benchmark and far from the near-complete pass-through we estimate in chains once corrected. The

[18] To provide further evidence that the effect of the exchange rate change we estimate is causal, we use aggregate data from INDEC, classify the categories of the CPI into treatment and control, and estimate our baseline specification in 2017, as a placebo check. Appendix Figure E.18 shows that prices in the treatment and control groups did not change differently in 2017 when the exchange rate was very stable.

non-food control specification we report for chains in the *Periferia* subsample (Figure 5) cannot be replicated for independent supermarkets, because Scentia's main data extract does not include non-food items for independents. With the two corrections we can implement, the qualitative pattern is unchanged: pass-through in independent supermarkets remains incomplete and asymmetric for both the VAT cut and its re-introduction.

Income effects. The VAT reform caused a large price decline across a substantial share of the household consumption basket, particularly for low-income households. This raises the possibility that some of the effects we estimate may reflect income effects, whereby consumers' budget constraints are relaxed because many of the goods they purchase are now cheaper. Income effects are notoriously difficult to isolate and are often assumed away in the Public Finance literature (Saez et al., 2012). However, if income effects impact both control and treatment goods similarly, their net effect would be differenced out in our estimation framework.

We address this concern in two ways. First, we test for aggregate income effects using semi-aggregate VAT return data. If income effects were substantial, we would expect to see an increase in total sales in the grocery sector relative to other retail sectors around the reform (because untreated goods are normal goods and their consumption should rise as effective income rises). As shown in Appendix Section C, total sales in the grocery sector do not change differentially around the reform, suggesting that any aggregate income-effect spillover is small. We note that this test is suggestive rather than definitive: with only nine retail sectors and one treated sector, the standard errors leave room for moderately sized effects.

Second, we exploit store–barcode level data from the Córdoba sample to test for income effects by comparing sales of untreated goods (i.e., goods not affected by the VAT cut) between chain and independent supermarkets. We focus on Córdoba because it is one of Argentina's largest provinces, the estimated pass-through rates there closely match our national results, and purchasing disaggregated data for all stores in that province was feasible within our budget. This exercise is especially well-suited for detecting income effects, because the pass-through of the VAT cut was much higher in chains than in independents, meaning consumers shopping at chains experienced a larger effective income shock. If income effects were present, we would expect higher demand for untreated items in chain supermarkets relative to independents, even though these goods were not directly affected by the VAT cut. The key identifying assumption is that consumers do not switch store types in response to the reform.

Appendix Figure E.20 presents the results for untreated goods and, for comparison, also includes the same analysis for treated products. We find no evidence of a differential increase in prices or quantities of untreated goods between chain and independent stores following the VAT cut. Conversely, quantity effects for treated goods are large, as expected given the higher pass-through in chains. This pattern strengthens our interpretation: the VAT reform produced large direct effects, but little to no indirect demand spillovers via income effects. We cannot fully separate income effects from within-store substitution, however: substitution from untreated toward the now-cheaper treated goods could in principle generate a similar cross-store pattern, so the Córdoba evidence is best read as showing that the joint contribution of these two channels to our estimates is small, rather than as isolating either individually. The exercise nonetheless holds constant the item barcode and region, while exploiting variation in effective treatment intensity across store types—a powerful identification strategy in the price incidence literature.

Sample restriction. The main estimation sample restricts to a balanced panel of barcodes with positive weekly/monthly sales between January 2019 and March 2020. Because this conditions on positive sales, it raises two potential concerns: that pass-through estimates may be biased if dropped barcodes differ systematically across treatment and control groups, and that the reform may have affected the extensive margin of product availability. We address both concerns in Appendix E.1. First, re-estimating our specification on the unbalanced sample, which retains all barcodes rather than only those continuously present, leaves the pass-through estimates essentially unchanged in both chain and independent supermarkets (Appendix Figure E.21). Second, a monthly difference-in-differences on an indicator for whether a barcode is actively sold shows no differential post-reform exit of treated relative to control goods (Appendix Figure E.22), and treated and control barcodes exhibit very similar survival profiles (Appendix Figure E.23). Together these checks indicate that the balanced-panel restriction does not drive our results and that the reform had no differential effect on product availability.

Winsorization. A separate concern in our high-inflation setting is whether the pass-through estimates are sensitive to extreme observations in the tails of the price-change distribution. We verify that they are not: the dynamic difference-in-differences estimated on the price outcome winsorized at the 1st and 99th percentiles is nearly indistinguishable from the unwinsorized baseline throughout the event window, including the pre-reform months (Appendix Figure E.24).

Weighting and aggregation. Our pass-through estimates are robust to alternative weighting and aggregation schemes, as we show in three ways. Within each (barcode, region, store-type) cell, the prices we observe from Scentia are unit values (total expenditure divided by total quantity), so the within-cell aggregation is already quantity-weighted by sales. Across cells, our baseline specification does not apply additional weights. First, we re-estimate after replacing the equal weighting of local-market cells with sales weights, in two ways: separately by store type, aggregating each barcode's sales across regions into a national sales-weighted price (Appendix Figure E.25a), and pooling across store types into a single sales-weighted national price per barcode (Appendix Figure E.25b). In both cases, the sales-weighted estimates are very close to the baseline. Second, Appendix Figure E.26 re-estimates the main specification using monthly aggregate price series from the National Institute of Statistics and Censuses (INDEC), which constructs these series with the representative expenditure weights it uses to compute the official Consumer Price Index. Because INDEC aggregates across all store types using these external weights, it provides an independent check under a weighting scheme entirely different from our own; the dynamic difference-in-differences estimated on the INDEC data closely tracks the pattern we report in the main text. Third, Appendix Table A4 reports a leave-one-out exercise that partitions the sample by region, treated-goods category, store type, and data frequency. The pass-through estimates are highly stable across all partitions, indicating that no single good or region drives our main result.

Cross-store evidence on the uncapped-goods counterfactual. A concern with our comparison of capped and uncapped treated goods within chains is that, under the monitoring environment we describe in Section 2, chains may have shifted profit-making across goods: a price increase on capped goods would be more conspicuous to the regulator than one on uncapped goods, so stores most exposed to the caps might raise prices on uncapped goods to compensate for the cap-induced losses. We examine this directly using cross-store variation in pre-reform exposure to the caps. We use store-level data for Córdoba, covering ninety chain stores in the province at the monthly frequency, and compute each store's pre-reform capped-goods share using sales from January to July 2019. Appendix Figure E.27a shows that the pre-reform share of total sales accounted for by capped goods varies meaningfully across stores, ranging from roughly 5% to roughly 13%. We sort stores into Low, Middle, and High intensity terciles by this pre-reform capped-goods share (each tercile contains the same number of stores), and re-estimate our dynamic difference-in-differences specification (1) separately within each tercile.

Appendix Figure E.27b reports the results for uncapped treated goods. Across the three terciles, the pass-through of the VAT cut is similar. After the VAT is re-introduced, the three terciles behave similarly: there is no tendency for the most cap-exposed (High-intensity) stores to raise uncapped-goods prices more than less-exposed stores. This is the opposite of what the within-store profit-shifting mechanism would predict, under which the stores hit hardest by the caps would have raised uncapped-goods prices by more. Appendix Figure E.27c repeats the exercise for capped treated goods, which shows the anti-profiteering caps binding: in all three terciles the price response to the VAT re-introduction is held well below the full pass-through benchmark.

5 Consumer Surplus Effects of the Temporary VAT Cut

The official goal of the VAT cut was to preserve low-income households' access to a basic basket of necessities during a period of unusually high inflation, driven by a sharp depreciation of the Argentine peso following unexpected election results. In this section, we quantify the consumer surplus and distributional effects caused by the temporary VAT cut and assess the extent to which it achieved its intended goals.

A full social-welfare framework would integrate consumer surplus with producer surplus, government revenue, and welfare weights. We do not attempt such a full quantification in this paper for two reasons. First, we do not directly observe marginal cost or its evolution across stores and goods, which makes a structural decomposition of producer surplus difficult to identify in our data; the equilibrium and efficiency effects of the price-cap component of the policy, including producer surplus, are taken up in a companion paper that uses a fully structural industrial-organization approach.[19] Second, the anti-profiteering caps in our setting created an asymmetric distortion in the opposite direction — capped chain goods settled persistently below their pre-VAT-cut levels relative to control (Section 4.2) — which is itself a pitfall of imposing ex-ante price caps without information on marginal costs. A consumer-surplus framework does not fully capture this efficiency cost; we discuss the implications informally in the closing of this section and in the Conclusion.

5.1 Data

Ideally, we would like to observe each shopper's income at every supermarket to account for differences in shopping behavior and basket composition across income groups. Since such data are not available, we complement our analysis with the household expenditure survey data (ENGHo) described in Section 3.1. In particular, we use ENGHo's consumption data to estimate the share of total expenditure on goods affected by the VAT cut and subsequent anti-profiteering caps, as well as the types of supermarkets households shop at.

5.2 Stylized Facts

Panel (a) of Figure 6 reports, for each income decile, the share of treated (zero-rated) goods in total household expenditure (solid black line, right axis) together with the store types in which households buy them (stacked bars). The expenditure share decreases with income, from roughly 10% for the lowest decile to roughly 2% for the richest (national average: 4.6%), indicating that treated goods make up a larger share of low-income households' budgets. The stacked bars show that the lowest-income decile makes 48% of its treated-good purchases in independent stores and 22% in chain supermarkets; the chain share rises with income while the independent share declines (specialized stores, such as butcheries and bakeries, are not included in our scanner data and were not directly part of the VAT change; similarly, purchases from street vendors are negligible in our setting). In absolute terms, however, household expenditure on zero-rated goods in pesos rises with income (Appendix Figure E.28), so richer households may benefit more from the subsidy on a level basis. We investigate these effects more formally in Section 5.3 below.

Panel (b) of Figure 6 decomposes the share of total expenditure on treated goods into goods subject to the anti-profiteering caps and goods exempt from them, separately for chain and independent supermarkets. Non-capped treated goods account for a small share of total expenditure throughout the income distribution. The share spent on capped goods declines with income in both

[19] *Equilibrium Effects of Voluntary Price Control Policies*, by Barahona, Benzarti, Díaz de Astarloa, García-Lembergman, Garriga, Jiménez-Hernández, and Tortarolo (in progress).

store types and is markedly larger at independent supermarkets—falling from roughly 5.7% for the lowest decile to 0.7% for the highest, versus 2.2% to 1.2% at chains. Because the caps applied only at chain supermarkets, this gradient underlies the cap-related distributional patterns we quantify in Section 5.3.

This finding, that the propensity to purchase food items at chain supermarkets increases with household income, coupled with the fact that the pass-through of the VAT cut in chain supermarkets was more than twice that of independent supermarkets, implies that the VAT cut likely benefited richer households more. While low-income households benefited from the VAT cut, both because some of them shop at chain supermarkets and because independent supermarkets did pass through some of the VAT cut, this evidence implies that there was room to better target the VAT cut.

Next, we estimate a consumer surplus model to formally quantify the distributional effects of the VAT cut. The model also serves as a guide for identifying which outcomes should be estimated and which are irrelevant for consumer surplus. For instance, it shows that tax evasion need not be estimated separately, as long as its effects are fully reflected in observed price changes.

5.3 Quantifying the Effects of VAT Changes on Consumers

In this section, we propose a consumer surplus model to assess the impact of VAT changes on households as a function of moments observed in our data. The model is adapted from the trade and development literatures on the welfare effects of government policies estimated from scanner-level data (e.g., Atkin et al., 2024, 2018; Redding & Weinstein, 2020; Handbury, 2021; Faber & Fally, 2022). It is based on a nested Constant Elasticity of Substitution (CES) utility function; while some of its assumptions might be strong (as we discuss below), this framework is tractable and parsimonious.

We adapt the nested CES approach from Atkin et al. (2018), which uses a three-tier demand system. Consider a household in income decile h with income w_h. In the upper tier, we assume Cobb-Douglas preferences over product groups $g \in G$ (e.g., coffee); in the middle tier, we assume asymmetric CES preferences over local retailers $s \in S$ (e.g., supermarket chains and independent grocery stores) that sell the product group considered; and in the final tier, we assume preferences over individual products within product groups $b \in B_g$ (e.g., NESCAFÉ Clásico Instant Coffee, Dark Roast, 7 oz. Jar):

$$U_h = \prod_{g \in G} (Q_{gh})^{\alpha_{gh}} \tag{2}$$

$$Q_{gh} = \left[\sum_{s \in S_g} \beta_{gsh} q_{gsh}^{\frac{\sigma_{gh}-1}{\sigma_{gh}}} \right]^{\frac{\sigma_{gh}}{\sigma_{gh}-1}} \tag{3}$$

$$q_{gsh} = \left[\sum_{b \in B_g} \beta_{gsbh} q_{gsbh}^{\frac{\sigma_{gsh}-1}{\sigma_{gsh}}} \right]^{\frac{\sigma_{gsh}}{\sigma_{gsh}-1}} \tag{4}$$

where α_{gh}, β_{gsh}, and β_{gsbh} are income group-specific preference parameters that are fixed across periods; Q_{gh} and q_{gsh} are product group and store-product group consumption aggregates with associated price indexes P_{gh} and r_{gsh}, respectively; σ_{gh} is the elasticity of substitution between product group g across chain and independent supermarkets; and σ_{gsh} is the elasticity of substitution between individual products b within store and product groups. For each broad product group g (e.g., coffee), consumers choose how much to spend at different stores based on the store-level price index r_{gsh}, which itself depends on the products they anticipate buying in each store, given its specific product mix and product-level prices.

Using the CES properties, we derive the following indirect utility function: $V_h = w_h/P_h$, where w_h is income and P_h is the CES ideal price index defined by

$$P_h = \prod_g P_{gh}^{\alpha_{gh}},$$
$$P_{gh} \equiv \left(\sum_s \beta_{gsh}^{\sigma_{gh}-1} P_{gsh}^{1-\sigma_{gh}} \right)^{\frac{1}{1-\sigma_{gh}}},$$
$$P_{gsh} \equiv \left(\sum_b \beta_{gsbh}^{\sigma_{gsh}-1} p_{gsbh}^{1-\sigma_{gsh}} \right)^{\frac{1}{1-\sigma_{gsh}}}.$$

We can estimate the indirect utility change as follows:

$$d \ln V_h = d \ln w_h - d \ln P_h, \tag{5}$$

Ignoring the income effects yields:

$$d \ln V_h = -\sum_g \alpha_{gh} d \ln P_{gh}, \tag{6}$$

where α_{gh} is the initial expenditure share of household h in varieties from group g. Differentiating P_{gh} with respect to component prices and simplifying yields

$$d \ln P_{gh} = \sum_s \alpha_{gsh|g} \, d \ln P_{gsh}, \tag{7}$$

where $\alpha_{gsh|g}$ is the initial expenditure share in products from group g at store type s. Performing the same analysis to P_{gsh} in the third nest, we obtain:

$$d \ln P_{gsh} = \sum_b \alpha_{gsbh|s_b} d \ln p_{gsbh}. \tag{8}$$

Substituting equation (7) and (8) into (6) yields the following key consumer surplus expression:

$$d \ln V_h = -\sum_{gsb} \underbrace{\alpha_{gh}\alpha_{gsh|g}\alpha_{gsb|s_b}}_{\alpha_{gsbh}} d \ln p_{gsbh}, \tag{9}$$

where α_{gsbh} corresponds to the initial expenditure share of household h on product b from product group g at store type s. While the demand system is homothetic, we capture potential heterogeneity across the income distribution by allowing income deciles to differ in their preferences for consumption bundles across stores and product groups, and in their expenditure shares across product groups and store types (Atkin et al., 2018). The change in surplus only depends on the *initial* expenditure shares and the change in tax-inclusive prices p_{gsbh}. Therefore, to a first-order approximation, there is no need to estimate the elasticities of substitution or any changes in the expenditure shares; we show in Appendix D that the reform-induced changes in expenditure shares are second-order and therefore do not affect these estimates.[20]

This nested CES model is tractable and easily mapped onto the data, but presents three limitations worth emphasizing. First, it captures the immediate impact of the policy and does not account for longer-term effects. Because the effects of the VAT increase are longer-lived than those of the VAT decrease, we are likely underestimating the effects of the VAT increase relative to the VAT cut; for this reason, we do not attempt to quantify the *net* impact of the policy. Second, our model focuses on consumers and does not account for changes in firm profits or government revenue; we discuss these channels separately in Section 5.6.1. Third, our approach suffers from the so-called "missing intercept" problem (e.g., see Adão et al., 2019; Wolf, 2023): it may not recover the aggregate, general-equilibrium impact of the VAT shock if product groups or stores are interconnected through demand spillovers or substitution (Minton & Mulligan, 2024). A fully structural approach is beyond what we attempt in this paper.

In all, while our CES approach imposes a particular structure on household demand, it yields a simple and tractable formula (9) that solely depends on observable expenditure shares and tax-inclusive price changes.[21] In this sense, the approach is equivalent to a Laspeyres price index, which uses initial consumption weights $(q \cdot p'/q \cdot p)$. In practice, for each decile h, we multiply the observed pre-reform expenditure share of food group g at store type s (independent or chain), α_{gsh}, by the estimated price change of food group g at store type s induced by the VAT change, $d \ln P_{gsh}$. For the VAT cut (Panel (a) of Figure 7), we use estimates that pool capped and uncapped treated goods within each store type, because the cap distinction only becomes relevant at the repeal. For the combined VAT cut and reintroduction (Panel (b) of Figure 7) and the no-caps simulation, we use estimates that consider capped and uncapped goods separately within chain supermarkets, in order to engage with the asymmetric-in-opposite-directions pattern documented earlier (the caps held

[20]See Baqaee & Farhi (2019) for a similar argument used in macroeconomics to study the effect of productivity shocks at the national level.

[21]While we derive equation (9) under CES preferences, the same expression holds for any continuous utility function representing locally non-satiated, convex, and homothetic preferences, as shown in Appendix D.2.

capped-chain prices below the uncapped counterfactual). The corresponding expenditure shares are taken from the household survey, as described above. For each decile h, the total consumer surplus change is the sum of these moments across the g food groups and store types (independent and chain). We perform this exercise separately for the VAT cut and the VAT increase.

Note that many features of this environment are not explicitly modeled (such as tax evasion). However, these effects are accounted for by our model as long as they are captured by price changes. For example, if retailers evade VAT, the price response to VAT changes will be muted (see Appendix Section B.3.1). Therefore, the effect of evasion decisions would indeed be picked up by our reduced-form price-VAT pass-through estimates. In other words, the retail-product-specific price changes and consumption shares across categories in chain and independent supermarkets would reflect any behavior that is reflected in prices and quantities (which is true for most economic behavior), without requiring us to model it explicitly, similarly to a "revealed preference mechanism".

5.4 Distributional Effects on Consumers

Figure 7 plots the results of quantifying our consumer surplus equation (9) for the VAT cut (Panel a) and the combined VAT cut and its reintroduction (Panel b), by income deciles. The estimates are measured in real income, and thus the y-axis in these figures corresponds to a percentage change in real income. In addition, both of these estimations are expressed relative to the pre-reform situation and focus on *impact*, i.e., they consider only the effect of VAT changes one month after they occur.

Panel (a) plots the *observed* effect of the VAT cut alongside a simulation assuming full pass-through to prices.[22] We find that the VAT cut benefited consumers and was markedly progressive, with gains for the lowest income decile about three times larger than for the highest. This is mostly because the expenditure share of zero-rated goods for low-income households is substantially higher than that of higher-income households. Consumer surplus falls substantially short of the full-pass-through simulation, however, and the shortfall is larger for lower-income households: the observed gain for the lowest decile is 0.6% of real income against a simulated 1.4%, while for the highest decile the corresponding figures are 0.2% and 0.3%. This gap is fully explained by the limited pass-through in independent grocery stores, which is precisely where low-income people tend to shop the most: in chain supermarkets, the substitution and peso-depreciation corrections put pass-through of the VAT cut at the full pass-through benchmark (Figure 5), so the shortfall is entirely attributable to the independent-store leg of the model.

Panel (b) plots the combined effect of the VAT cut and its reintroduction. Overall, the impact of the VAT increase, combined with the anti-profiteering measures, was regressive, hurting low-income households more than high-income households. This is mostly because low-income households tend

[22]The simulation does not account for tax evasion, which would likely lead to a smaller effect (since the price pass-through would be smaller under evasion as shown in Appendix Section B.3.1).

to shop at independent stores, where prices exhibit more asymmetric pass-through, while high-income households shop at supermarket chains, where the anti-profiteering measures compressed price increases.

We compare these observed effects to a simulation assuming no anti-profiteering measures. We construct this simulation by applying our estimates of pass-through rates for goods that were not subject to the anti-profiteering measures to those that were. Both types of goods followed similar trends prior to the VAT increase, so we believe our approach is reasonably sensible. We find that, absent the anti-profiteering measures, the impact of the VAT increase would have been significant and regressive, affecting the lowest-income decile about four times more than the highest decile. Given that the observed effect is substantially higher for all income deciles and somewhat similar across them, the anti-profiteering measures appear to have benefited low-income deciles substantially more. This pattern is consistent with cross-decile evidence on expenditure shares: Figure 6b shows that low-income deciles spend a larger share of total household expenditure on capped goods at chain supermarkets than higher-income deciles do, so the caps (which apply only at chain supermarkets) compress a regressive shock that would otherwise have fallen more heavily on the lowest-income deciles. And because our estimates do not account for the observed price hysteresis, the effect of the anti-profiteering measures is likely to be underestimated, i.e., we would expect the simulation estimates with no anti-profiteering measures to be substantially more negative when accounting for longer-run effects.

5.5 Effect on Government Revenue

Beyond its effects on consumers, the temporary VAT cut also reduced government revenue. To quantify this fiscal cost, we use a macro-level difference-in-differences approach applied to VAT return data. We summarize our approach here and provide more details in Appendix Section C. First, we create a panel dataset of VAT returns for the nine retail sectors reported in the VAT revenue data. Our main outcome of interest is the net VAT liability for each sector, which allows us to estimate the VAT revenue lost due to the VAT cut. One of the nine sectors is the grocery store sector (which pools chain and independent supermarkets), so we compare it to the remaining eight.

We estimate that the temporary VAT cut led to a 30% reduction in VAT remitted by grocery retailers in 2019 relative to other retail sectors (see Appendix Figure E.29). As a benchmark, it is useful to compare this effect with the sharp increase observed in 2020, which is explained by the COVID-19 pandemic, during which grocery stores remained open while other retailers (e.g., electronics, hardware, clothing) were subject to lockdown restrictions. Reassuringly, the two groups exhibit parallel pre-trends and no discernible differences from 2021 onward.

The VAT-return data also record total grocery-sector sales, which were roughly flat in 2019 (Appendix Figure E.34). Because these data are denominated in pesos (price $\times$ quantity) rather than quantities, a flat peso aggregate is consistent with the quantities of zero-rated goods rising—as the Córdoba evidence in Section 4.4 suggests—even as lower zero-rated prices and the reclassification of taxable to zero-rated sales hold total peso sales steady. The test thus rules out a large jump in peso-denominated grocery-sector sales, not a recomposition in quantities.

According to official estimates, the tax expenditure (i.e., forgone revenue) of this reform was equivalent to 0.07% of GDP over the four months of the cut, or 0.21% of GDP when annualized over the full year. For context, Argentina's conditional cash transfer program (AUH), which is its most important welfare program, costs 0.4 to 0.6% of GDP annually—meaning the VAT cut carried a fiscal cost equivalent to between one-third and one-half of the country's flagship antipoverty program.

5.6 Model Extensions

We consider one extension to the model introduced in Section 5.3 in the main text—incorporating income shocks from supermarket profits—and develop two further extensions in Appendix D: a generalization beyond CES preferences and the treatment of second-order changes in expenditure shares.

5.6.1 Incorporating Income Shocks for Shareholders

Supermarket profits (and losses) due to the temporary VAT cut should, in principle, be redistributed to those consumers who own supermarkets or hold shares in the grocery retail sector, which may affect the consumer surplus estimates reported in Section 5.4. Given that wealth is typically concentrated at the top of the income distribution, accounting for profits in the estimates may predominantly affect the top-income decile and potentially alter our conclusions about the distributional effects of the temporary VAT cut.

To assess whether this matters, we extend the model to let household income vary with firm profits—so that households with larger equity holdings see income rise or fall alongside supermarket profits—and trace the effect through to indirect utility; the derivation is in Appendix D.3. Estimating the resulting expression directly is difficult, as it requires changes in prices and quantities for every good and the distribution of supermarket ownership across households. Rather than implementing it, we bound its importance by showing that supermarket ownership is limited even for the top income decile—possibly because the wealth distribution in Argentina is highly skewed, or because several chains are foreign-owned (such as Carrefour and Walmart).

We use data from the household expenditure survey described above to estimate the ownership of "retail and wholesale" firms by income decile. The dataset does not contain a specific grocery retail category and so the estimates are likely to be an upper bound. We find that fewer than 2% of individuals in the top decile own any firms in the "retail and wholesale" sector (Appendix Figure

E.30a).

Similarly, we plot the distribution of business and capital income as a share of total income in Appendix Figure E.30b. Even for the top income decile, only 4.5% of total income is derived from business or capital ownership.[23] These estimates are also an upper bound, since they include ownership of any business or capital and are not restricted to grocery stores. Overall, this evidence mitigates the concern that supermarket profits are likely to qualitatively affect our conclusions about the distributional effects of the temporary VAT cut and anti-profiteering measures.

Taken together, the consumer-surplus and government-revenue results above provide a partial picture of the welfare effects of the temporary VAT cut and the anti-profiteering caps. A full social-welfare framework would also integrate producer surplus and the equilibrium response of supermarket pricing strategies, which we take up in the companion paper noted above. The qualitative reading that emerges is that the caps deliver a consumer-surplus gain relative to a no-caps counterfactual, but at the cost of creating an asymmetric distortion in the opposite direction for capped goods, which a static consumer-surplus framework does not fully capture.

6 Conclusion

Our findings have policy implications for the ubiquitous temporary VAT cuts implemented in L&MICs. First, we find that near-complete pass-through of VAT cuts is achievable in monitored settings: in chain supermarkets in this paper, once we correct for substitution across goods and a concurrent peso depreciation, the pass-through of the VAT cut is indistinguishable from the full pass-through benchmark of 17.4 percentage points (Figure 5). This is notable because the previous literature mostly finds that the pass-through of VAT cuts is incomplete. In contrast, independent stores, which were not subject to monitoring or anti-profiteering measures, replicate the findings of the previous VAT-cut pass-through literature: pass-through is incomplete for both the cut and the repeal, with the repeal pass-through larger, so that prices for goods treated by the VAT cut but not subject to anti-profiteering caps remain persistently above their pre-cut levels.

In a setting where uncapped pass-through of the VAT cut is already near-complete in chain supermarkets, the asymmetry concern motivating anti-profiteering measures is not incomplete pass-through on the cut but the repeal: it is at the repeal, in the less-monitored independent stores, that incomplete and asymmetric pass-through leaves prices above their pre-cut level. The persistent above-pre-cut prices we estimate for uncapped goods in independent supermarkets reflect this asymmetry.

Second, the government can use anti-profiteering measures to affect the pass-through of VAT changes to prices. However, this policy tool can come with pitfalls possibly leading to additional price distortions and efficiency costs. This is apparent in the case of chain supermarkets, whereby

[23] These figures are consistent with what researchers in the top incomes literature have documented for Argentina (see Alvaredo, 2010).

the anti-profiteering measures led to the opposite asymmetric effects with post-reform prices settling below pre-VAT cut levels.

Finally, our findings reveal three puzzling facts about how prices respond to taxes and regulatory shocks. First, prices respond asymmetrically to the VAT decrease and its repeal in uncapped goods — a pattern that canonical tax incidence models do not predict and that the usual price-rigidity explanations cannot rationalize in our high-inflation setting. Menu-cost macro pricing models can generate asymmetry of this type, helping to discipline which classes of models are most relevant. Second, prices of uncapped treated goods exhibit medium- to long-run hysteresis after the VAT repeal, remaining persistently above their pre-VAT-cut levels rather than converging back. Third, the cap-induced gap between capped and uncapped goods in chain supermarkets persists essentially unchanged for at least nine months after cap enforcement was effectively suspended in March 2020 — even though the same firms are demonstrably capable of rapid price adjustment and the caps were binding while in force. Both hysteresis findings pose a substantial challenge to existing macro pricing models, and we believe that explaining them will require new modeling tools.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens & Jeffrey M. Wooldridge. 2023. When should you adjust standard errors for clustering? *The Quarterly Journal of Economics* 138(1). 1–35.

Adão, Rodrigo, Costas Arkolakis & Federico Esposito. 2019. General equilibrium effects in space: Theory and measurement. Working Paper 25544 National Bureau of Economic Research.

Allcott, Hunt, Rebecca Diamond, Jean-Pierre Dubé, Jessie Handbury, Ilya Rahkovsky & Molly Schnell. 2019. Food deserts and the causes of nutritional inequality. *Quarterly Journal of Economics* 134(4). 1793–1844.

Alvaredo, Facundo. 2010. The Rich in Argentina over the Twentieth Century 1932-2004. *Top Incomes over the Twentieth Century: A Global Perspective* chapter 6.

Anderson, Simon P, Andre De Palma & Brent Kreider. 2001. Tax incidence in differentiated product oligopoly. *Journal of Public Economics* 81(2). 173–192.

Atkin, David, Benjamin Faber, Thibault Fally & Marco Gonzalez-Navarro. 2024. Measuring welfare and inequality with incomplete price information. *The Quarterly Journal of Economics* 139(1). 419–475. doi:10.1093/qje/qjad037.

Atkin, David, Benjamin Faber & Marco Gonzalez-Navarro. 2018. Retail globalization and household welfare: Evidence from mexico. *Journal of Political Economy* 126(1). 1–73. doi:10.1086/695476.

Bachas, Pierre, Lucie Gadenne & Anders Jensen. 2023. Informality, consumption taxes, and redistribution. *The Review of Economic Studies* doi:10.1093/restud/rdad095.

Bachmann, Rüdiger, Benjamin Born, Olga Goldfayn-Frank, Georgi Kocharkov, Ralph Luetticke & Michael Weber. 2026. A temporary VAT cut as unconventional fiscal policy. *The Review of Economic Studies* doi:10.1093/restud/rdag055.

Bajo-Buenestado, Raúl & Miguel Ángel Borrella-Mas. 2022. The heterogeneous tax pass-through under different vertical relationships. *The Economic Journal* 132(645). 1684–1708.

Ball, Laurence & David Romer. 1991. Sticky prices as coordination failure. *The American Economic Review* 539–552.

Baqaee, David Rezza & Emmanuel Farhi. 2019. The macroeconomic impact of microeconomic shocks: Beyond hulten's theorem. *Econometrica* 87(4). 1155–1203. doi:https://doi.org/10.3982/ECTA15202.

Benzarti, Youssef. 2025. Tax incidence anomalies. *Annual Review of Economics* 17. 615–634.

Benzarti, Youssef & Dorian Carloni. 2019. Who Really Benefits From Consumption Tax Cuts? Evidence From a Large VAT Reform in France. *American Economic Journal: Economic Policy* 11(1). 38–63.

Benzarti, Youssef, Dorian Carloni, Jarkko Harju & Tuomas Kosonen. 2020. What goes up may not come down: asymmetric incidence of value-added taxes. *Journal of Political Economy* 128(12). 4438–4474.

Besley, Timothy J & Harvey S Rosen. 1999. Sales taxes and prices: An empirical analysis. *National Tax Journal* 157–178.

Blundell, Richard. 2009. Assessing the temporary vat cut policy in the uk. *Fiscal studies* 30(1). 31–38.

Buettner, Thiess & Boryana Madzharova. 2021. Unit sales and price effects of preannounced consumption tax reforms: Micro-level evidence from european vat. *American Economic Journal: Economic Policy* 13(3). 103–34.

Calvo, Guillermo A. 1983. Staggered prices in a utility-maximizing framework. *Journal of monetary Economics* 12(3). 383–398.

Cameron, A. Colin, Jonah B. Gelbach & Douglas L. Miller. 2008. Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics* 90(3). 414–427.

Caplin, Andrew S & Daniel F Spulber. 1987. Menu costs and the neutrality of money. *The Quarterly Journal of Economics* 102(4). 703–725.

Carbonnier, Clement. 2007. Who Pays Sales Taxes? Evidence From French VAT Reforms, 1987–1999. *Journal of Public Economics* 91(5). 1219–1229.

Chetty, Raj, John N. Friedman, Michael Stepner & Opportunity Insights Team. 2024. The economic impacts of COVID-19: Evidence from a new public database built using private sector data. *The Quarterly Journal of Economics* 139(2). 829–889. doi:10.1093/qje/qjad048.

Chouinard, Hayley & Jeffrey M Perloff. 2004. Incidence of federal and state gasoline taxes. *Economics Letters* 83(1). 55–60.

Cooper, Russell & Andrew John. 1988. Coordinating coordination failures in keynesian models. *The Quarterly Journal of Economics* 103(3). 441–463.

Crossley, Thomas F, Hamish Low & Matthew Wakefield. 2009. The economics of a temporary vat cut. *Fiscal studies* 30(1). 3–16.

D'Acunto, Francesco, Daniel Hoang & Michael Weber. 2022. Managing households? expectations with unconventional policies. *The Review of Financial Studies* 35(4). 1597–1642.

Delipalla, Sophia & Owen O'Donnell. 2001. Estimating tax incidence, market power and market conduct: The european cigarette industry. *International Journal of Industrial Organization* 19(6). 885–908.

DellaVigna, Stefano & Matthew Gentzkow. 2019. Uniform pricing in us retail chains. *The Quarterly Journal of Economics* 134(4). 2011–2084.

Doyle, Joseph J & Krislert Samphantharak. 2008. $2.00 Gas! Studying the Effects of a Gas Tax Moratorium. *Journal of Public Economics* 92(3). 869–884.

Faber, Benjamin & Thibault Fally. 2022. Firm heterogeneity in consumption baskets: Evidence from home and store scanner data. *The Review of Economic Studies* 89(3). 1420–1459. doi:10.1093/restud/rdab061.

Fuest, Clemens, Florian Neumeier & Daniel Stöhlker. 2024. The pass-through of temporary vat rate cuts: Evidence from german supermarket retail. *International Tax and Public Finance* 1–47.

Fullerton, Don & Gilbert E Metcalf. 2002. Tax Incidence. *Handbook of Public Economics* 4. 1787–1872.

Gaarder, Ingvil. 2018. Incidence and Distributional Effects of Value Added Taxes. *The Economic Journal* 129(618). 853–876.

Genakos, Christos & Mario Pagliero. 2022. Competition and pass-through: evidence from isolated markets. *American Economic Journal: Applied Economics* 14(4). 35–57.

Golosov, Mikhail & Robert E Lucas. 2007. Menu costs and phillips curves. *Journal of Political Economy* 115(2). 171–199.

Gopinath, Gita & Oleg Itskhoki. 2010. Frequency of price adjustment and pass-through. *The Quarterly Journal of Economics* 125(2). 675–727.

Handbury, Jessie. 2021. Are poor cities cheap for everyone? non-homotheticity and the cost of living across u.s. cities. *Econometrica* 89(6). 2679–2715. doi:https://doi.org/10.3982/ECTA11738.

Harju, Jarkko, Tuomas Kosonen & Oskar Nordström Skans. 2018a. Firm Types, Price-Setting Strategies, and Consumption-Tax Incidence. *Journal of Public Economics* 165. 48–72.

Harju, Jarkko, Tuomas Kosonen & Oskar Nordström Skans. 2018b. Firm types, price-setting strategies, and consumption-tax incidence. *Journal of Public Economics* 165. 48–72.

Hellerstein, Rebecca & Sofia B Villas-Boas. 2010. Outsourcing and pass-through. *Journal of International Economics* 81(2). 170–183.

Hong, Gee Hee & Nicholas Li. 2017. Market structure and cost pass-through in retail. *Review of Economics and Statistics* 99(1). 151–166.

Kir, Ajitesh. 2023. Profiteering under GST: Lessons from India, Australia and Malaysia. *British Tax Review* (3). 295–316.

Kopczuk, Wojciech, Justin Marion, Erich Muehlegger & Joel Slemrod. 2016. Does Tax-Collection Invariance Hold? Evasion and the Pass-Through of State Diesel Taxes. *American Economic Journal: Economic Policy* 8(2). 251–86.

Kosonen, Tuomas. 2015. More and Cheaper Haircuts After VAT Cut? On the Efficiency and Incidence of Service Sector Consumption Taxes. *Journal of Public Economics* 131. 87–100.

Kotlikoff, Laurence & Lawrence Summers. 1987. Tax Incidence. *Handbook of Public Economics* 2. 1043–1092.

Minton, Robert & Casey B Mulligan. 2024. Difference-in-differences in the marketplace. Working Paper 32111 National Bureau of Economic Research. doi:10.3386/w32111.

Nakamura, Emi & Jón Steinsson. 2013. Price rigidity: Microeconomic evidence and macroeconomic implications. *Annu. Rev. Econ.* 5(1). 133–163.

OECD. 2024. *Oecd economic surveys: Malaysia 2024*. Paris: OECD Publishing. doi:10.1787/e45ca31a-en. https://doi.org/10.1787/e45ca31a-en.

Poterba, James M. 1996. Retail price reactions to changes in state and local sales taxes. *National Tax Journal* 165–176.

Redding, Stephen J & David E Weinstein. 2020. Measuring aggregate price indices with taste shocks: Theory and evidence for ces preferences. *The Quarterly Journal of Economics* 135(1). 503–560. doi:10.1093/qje/qjz031.

Saez, Emmanuel, Joel Slemrod & Seth H Giertz. 2012. The elasticity of taxable income with respect to marginal tax rates: A critical review. *Journal of Economic Literature* 3–50.

Sidhu, Nancy Dayton. 1971. The effects of changes in sales tax rates on retail prices. In *Proceedings of the annual conference on taxation under the auspices of the national tax association*, 720–733. JSTOR.

Tait, Mr Alan A. 1988. *Value added tax: International practice and problems*, vol. 24. International Monetary Fund.

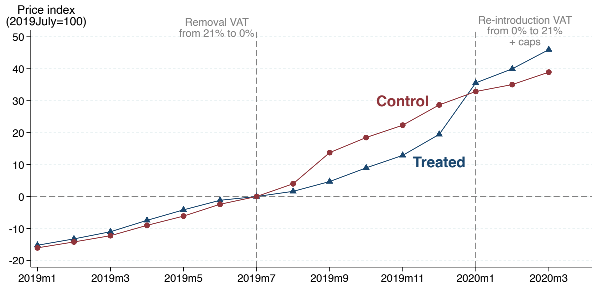
Weyl, E Glen & Michal Fabinger. 2013. Pass-Through as an Economic Tool: Principles of Incidence Under Imperfect Competition. *Journal of Political Economy* 121(3). 528–583.

Wolf, Christian K. 2023. The missing intercept: A demand equivalence approach. *American Economic Review* 113(8). 2232–69. doi:10.1257/aer.20211751.

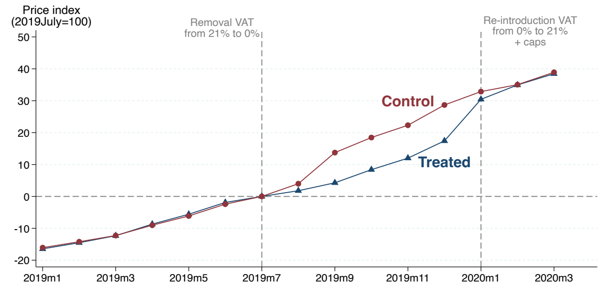
Figures and Tables

Figure 1: Price Effect of the Temporary VAT Cut, Pooling Chains and Independent Supermarkets

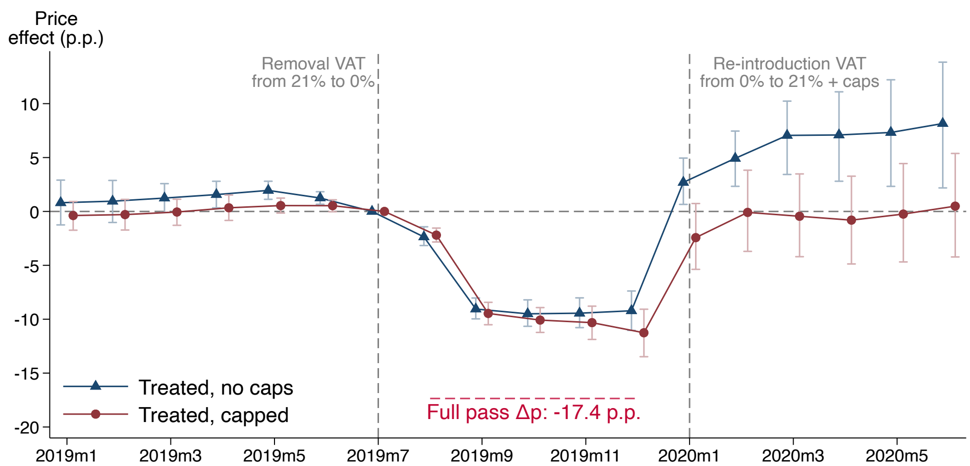
(a) **Unconditional Means (no caps)**



(b) **Unconditional Means (caps)**



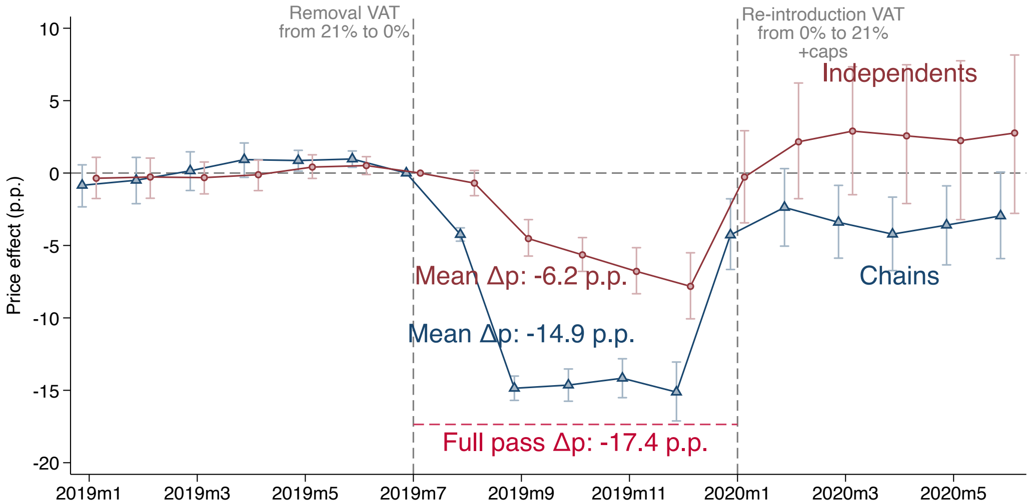
(c) **Difference-in-Differences Estimates (no caps vs. caps)**



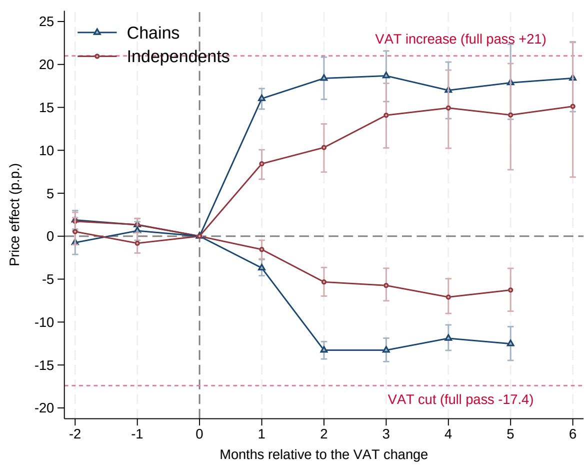
Notes: This figure shows the price effect of the temporary VAT cut and its repeal, pooling chain and independent supermarkets (monthly observations). Panels (a) and (b) plot the unconditional mean price level of treatment and control food products, separately before and after the VAT cut and its repeal, with each barcode series normalized to 100 in the month before the cut (July 2019); Panel (a) uses treated goods that were not subject to the anti-profiteering caps, and Panel (b) uses treated goods that were. Panel (c) plots the dynamic difference-in-differences estimates from equation (1) for the two groups on a common axis—the no-caps treated goods (navy) and the capped treated goods (maroon), each measured relative to the common control group of goods that remained at the standard 21% rate. We group barcodes into treatment and control as shown in Table 1. The first vertical dashed line marks the VAT cut to 0% and the second its reinstatement at 21% (with caps for the capped group). The red dashed line marks full pass-through to prices [(1-1.21)/1.21 x 100 = -17.4%]. Bands in Panel (c) are 95% wild cluster bootstrap confidence intervals.

Figure 2: Price Effect of the Temporary VAT Cut in Chain and Independent Supermarkets

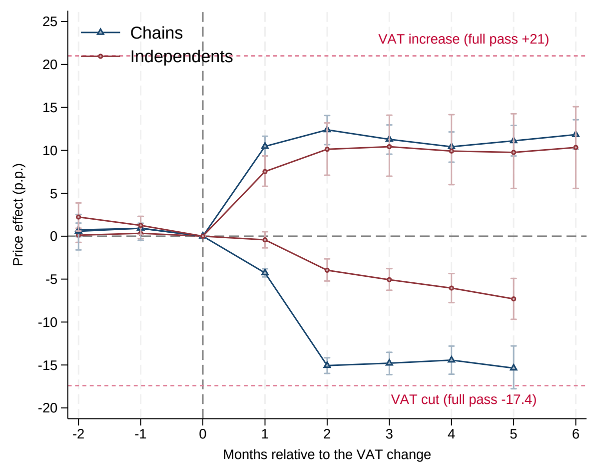
(a) **All treated goods: chains vs. independent supermarkets**



(b) **Uncapped goods (event time)**



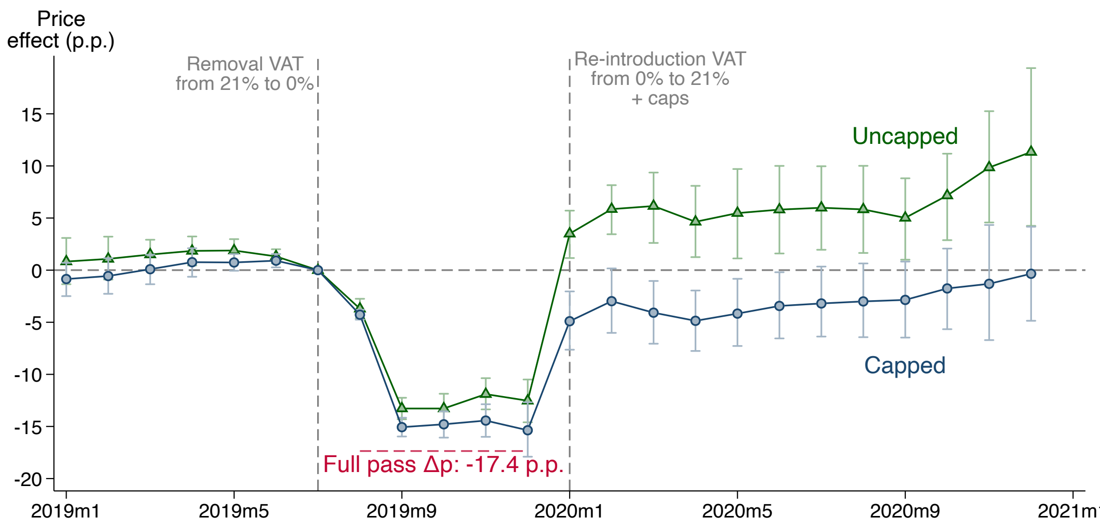
(c) **Capped-category goods (event time)**



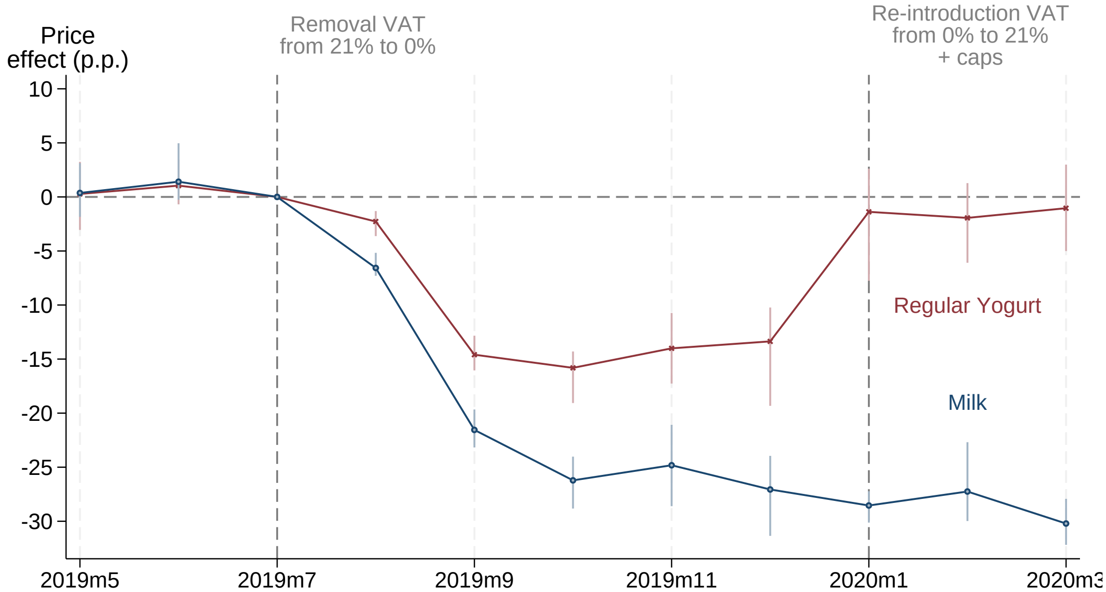
Notes: This figure plots the dynamic difference-in-differences coefficients from equation (1) comparing chain (navy) and independent (maroon) supermarkets; barcodes are grouped into treatment and control as in Table 1, and prices are normalized to 100 in the month before the VAT cut. Panel (a) uses all treated goods over the full sample: chains pass the VAT cut through close to fully and largely reverse it at the repeal, whereas independents pass through a smaller share of the cut and finish above their pre-cut level. Panels (b) and (c) re-center both reforms on a common event-time axis (months relative to each VAT change, with the cut shown as the downward series and the increase as the upward series) and split the treated goods by cap status: uncapped goods in Panel (b) and goods in the capped categories in Panel (c). The horizontal dashed lines mark full pass-through of the cut (-17.4 p.p.) and of the increase (+21 p.p.). The cap distinction is defined by the chain anti-profiteering list; independent supermarkets were never subject to caps, so for independents Panels (b) and (c) simply partition the treated goods by category. The first vertical dashed line indicates when the VAT was reduced to 0% and the second when it was reinstated at 21%. Bands are 95% wild cluster bootstrap confidence intervals; in Panel (a) they are offset horizontally for legibility.

Figure 3: Price Effect of the Anti-Profiteering Measures in Chain Supermarkets

(a) **7% cap versus no cap**

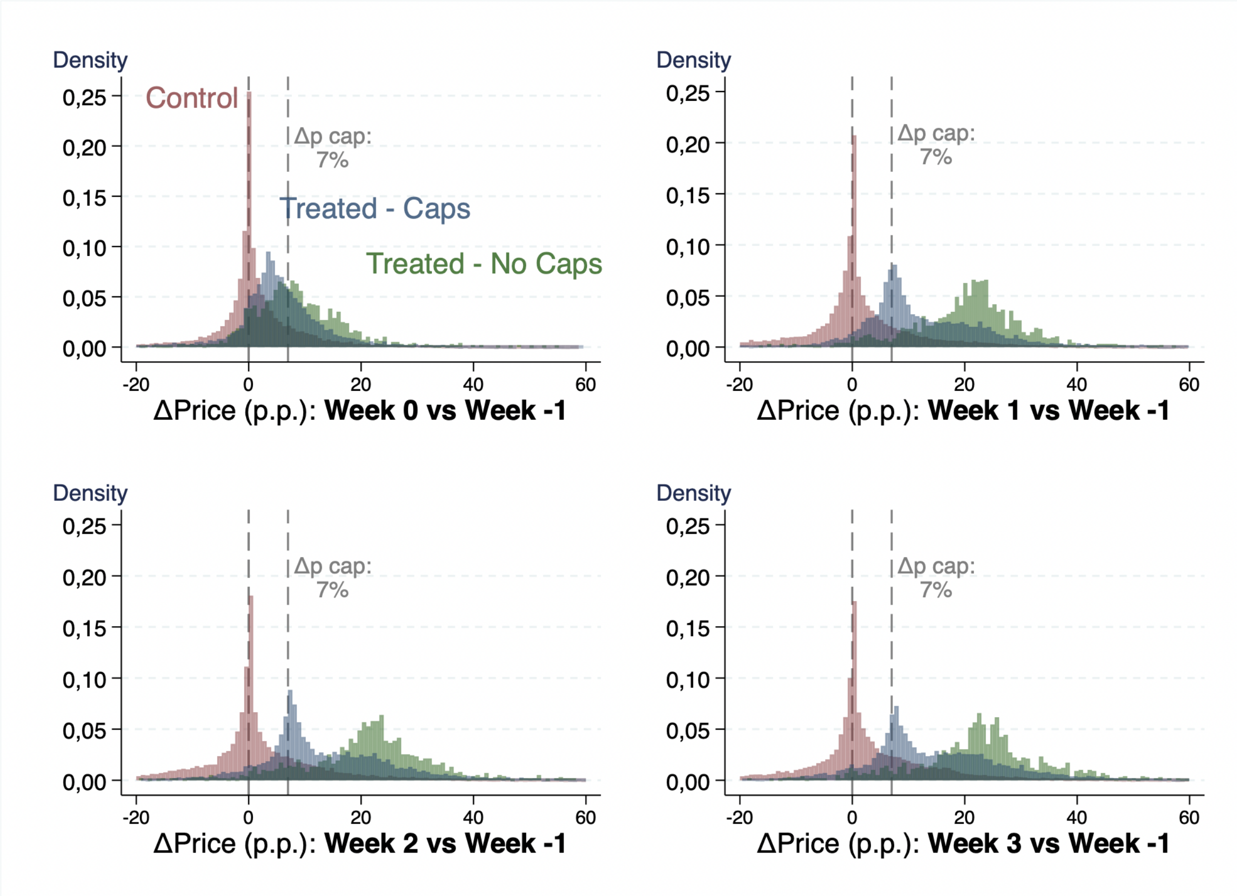


(b) **0% cap (milk) versus 7% cap (yogurt)**



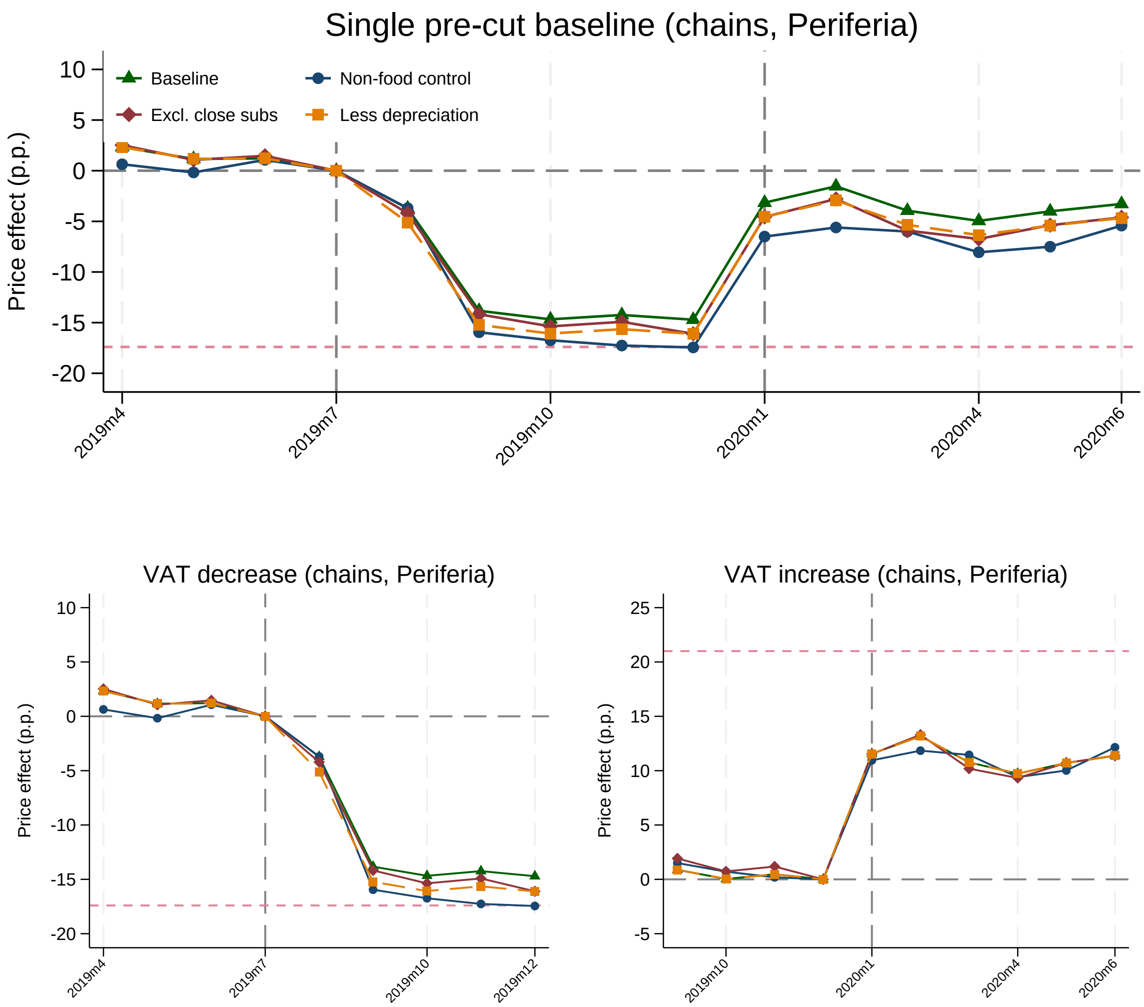
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) in supermarket chains before and after the VAT cut and its subsequent repeal. We break down the list of barcodes from the treatment group into food categories that are subject to a capped price increase and food categories with no cap on their price increase (i.e., those allowed to flexibly increase prices). We compare each group relative to food products in the original control group. For a list of the different caps across categories see Table 2. The dependent variable is the price of each barcode normalized to 100 in the week before the VAT was cut. Panel (a) compares the change in prices for those commodities that are subject to the 7% price increase cap and those that are fully flexible (relative to the original control group). Panel (b) compares the change in prices for milk products, which were not allowed to increase prices at all, relative to goods in the original control group. For comparison, in Panel (b), we also include the effect for regular yogurt that faced the 7% price increase cap. The first vertical dashed line indicates the time when the VAT was reduced to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps on allowed price increases.

Figure 4: Distribution of Price Changes in Chain Supermarkets Following the VAT Increase



Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase at different time horizons. Among treated goods, we exclude milk, which was subject to a 0% price increase cap. The reference week ($Week$ -1), refers to the last week of 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 0 denotes the first week with a rate of 21%, $Week$ 2 the second, and so on. In each panel, we include two vertical dashed lines to illustrate the locations of barcodes with no price change ($\Delta 0\%$) and those with a price change equal to the cap ($\Delta 7\%$). Longer horizons (weeks 4 through 11) are included in Appendix Figure E.11

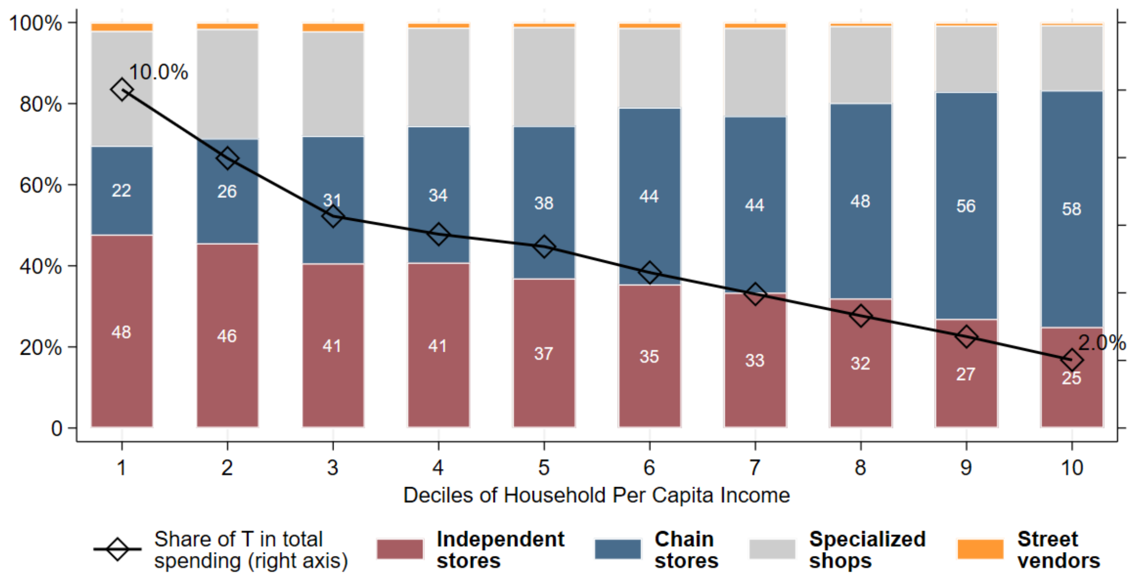
Figure 5: Price Effects in Chain Supermarkets Using Alternative Specifications



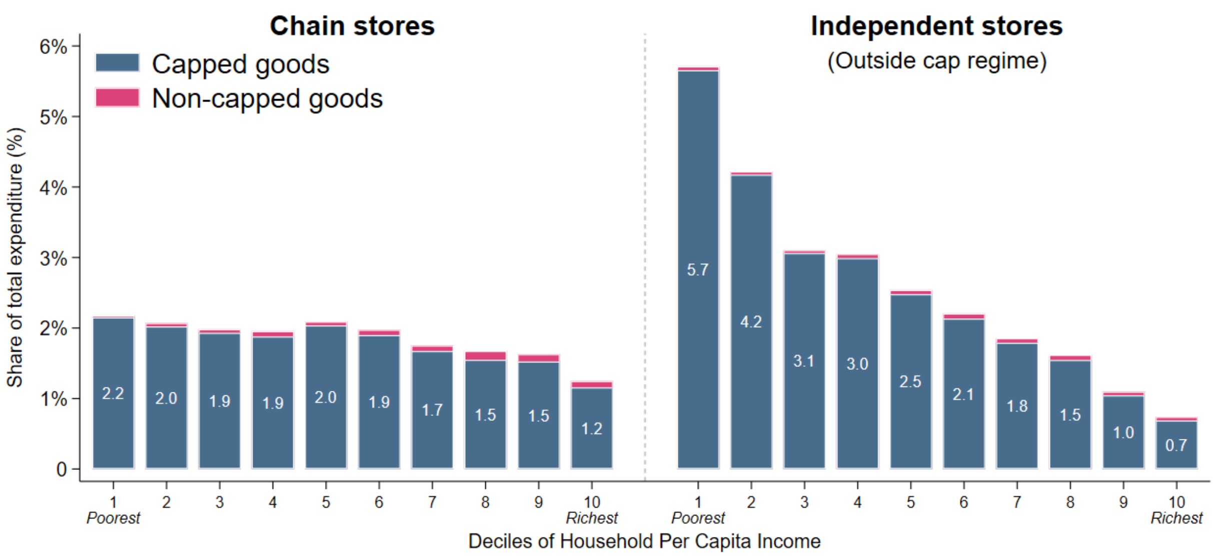
Notes: This figure plots the price effect of the VAT cut and its re-introduction in chain supermarkets in the *Periferia* region (monthly), under four specifications shown as separate series: baseline estimates; a control group made of non-food items only; a control group that excludes close substitutes; and baseline estimates netting out the effect of the currency depreciation. The estimates are restricted to *Periferia* because non-food data are only available for this region. The top panel measures all periods against a single pre-cut baseline (July 2019). The two bottom panels estimate each reform separately, normalized to its own pre-reform month: the VAT decrease relative to July 2019, and the VAT increase relative to December 2019 (shown through six months after the re-introduction). Vertical dashed lines mark July 2019 (the VAT cut) and January 2020 (the re-introduction); horizontal dashed lines mark full pass-through of the cut $[(1 - 1.21)/1.21 \times 100 = -17.4\%]$ and, in the increase panel, of the re-introduction (+21 p.p.).

Figure 6: Treated (zero-rated) Goods in Total Household Expenditure

(a) **Share of zero-rated goods in total expenditure, by income decile and store type**

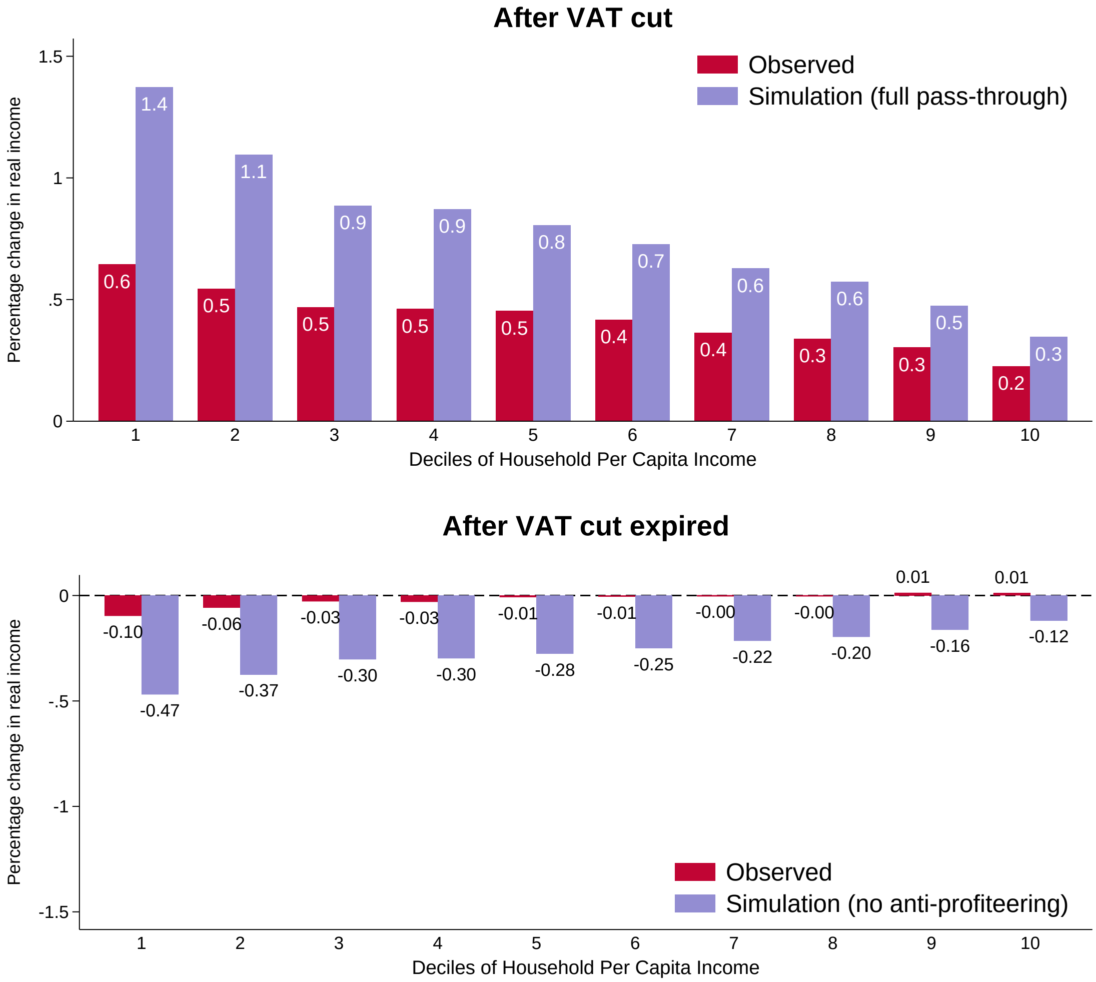


(b) **Spending on Capped and Non-Capped Treated Goods, by Income Decile and Store Type**



Notes: Panel (a) shows the share of zero-rated goods in total household expenditure by income decile (solid black line, right axis); the national average is 4.6% for treated food and 17.8% for untreated food. The stacked bars decompose zero-rated food spending across store types: independent supermarkets (maroon), chain supermarkets (navy), specialized shops such as butchers, greengrocers, and bakeries (gray), and street vendors and informal markets (orange). The corresponding per-capita peso amounts are reported in Appendix Figure E.28. Panel (b) decomposes the share of total household expenditure devoted to treated goods into items subject to the anti-profiteering price caps (navy) and items exempt from them (pink), separately for chain and independent supermarkets. Because Panel (a) computes shares from aggregate decile-level spending while Panel (b) averages household-level expenditure shares, the two panels are not directly comparable in levels. Income deciles are defined on per capita total household income.
Source: Authors' calculations using the 2017/18 National Household Expenditure Survey (ENGHo).

Figure 7: Consumer Surplus Estimates



Notes: These figures show the results of estimating our consumer surplus model (Section 5) separately for the VAT cut in Panel (a) and the combined VAT cut and its reintroduction in Panel (b). In Panel (a) we also include the consumer surplus effect for a simulation under full pass-through; in Panel (b), we additionally plot the simulated effect of the combined VAT cut and its reintroduction if anti-profiteering measures had been removed. Both panels measure effects relative to the pre-VAT-cut situation, on impact (one month after the relevant VAT change). The y-axis is in real-income percentage points.

Table 1: VAT Cut Treatment and Control Groups

Treatment
Temporary 0% VAT

Categories
Cooking oils (sunflower, corn, mix)
Rice
Dried pasta
Tea, Yerba Mate, and Mate Cocido
Sugar
Canned vegetables and beans
Canned fruits
Corn flour (*polenta*)
Wheat flour
Fluid milk (whole/skim)
Yogurt (whole or skim)
Eggs
Bread
Breadcrumbs and/or batter

Control
Standard 21% VAT

Categories
Other cooking oils (olive, soy, canola)
Rice-based meals
Breakfast cereal
Coffee
Salt
Herbs, Spices, & Seasonings
Dulce de leche (caramel)
Jam and Jelly
Other flours
Crackers, Biscuits, Toasts, Puddings
Chocolate
Mayonnaise
Vinegar
Dried legumes and beans

Notes: This table shows how the data is split into treatment and control categories. Wheat flour and Bread are taxed at the reduced rate of 10.5%. Source: Treatment categories are determined based on Decree 567/2019–Annex. Control products include the remaining categories in the data.

Table 2: Anti-Profiteering Measures

Treated: VAT back to 21%

Categories	Δp cap
Oil (sunflower & mix)	9%
Oil (corn)	No cap
Rice (regular: long grain white)	7%
Rice (other: basmati, brown, organic)	No cap
Dried pasta	7%
Tea, Yerba Mate, and Mate Cocido	7%
Sugar	7%
Canned vegetables and beans	7%
Canned fruits	No cap
Corn flour	7%
Wheat flour	7%
Fluid milk (whole/skim)	0%
Yogurt (regular)	7%
Yogurt (other: w/cereal, fruit chunks)	No cap
Eggs	7%
Sliced Bread (white)	7%
Sliced Bread (rest)	No cap
Breadcrumbs and/or batter	10.5%

Notes: This table shows the list of treated products with differential caps when the VAT rate was reverted back to its pre-VAT-holiday level of 21%. This mandate was announced on December 31 and enforced with the price monitoring app. "No cap" flags the uncapped food products with flexible prices. Capped products were permitted to increase their prices by up to $X\%$ of the December 31 price (7% for most of the goods). According to reports in newspapers, there was a contentious six-hour meeting on December 31 involving the government, producers, and supermarkets to discuss how the VAT increase would be reflected in prices. For more details, see La Nación's coverage here.

Table 3: Price Effects of the Temporary VAT Cut

Regions	Frequency	Stores	Goods	Estimates		Number of	
				Removal	**Re-intro**	**Barcodes**	**Observations**
All	Monthly	All stores	T: all goods	-10.377***	-0.170	7,832	612,115
				(0.591)	(1.594)		
			T: no caps	-9.216***	5.982***	4,574	376,422
				(0.604)	(1.380)		
			T: caps	-10.194***	-0.276	7,194	564,010
				(0.664)	(1.830)		
All	Monthly	Chain stores	T: all goods	-14.696***	-2.885**	4,530	319,896
				(0.572)	(1.289)		
			T: no caps	-12.741***	6.004***	2,763	201,485
				(0.624)	(1.340)		
			T: caps	-14.911***	-3.527**	4,167	296,063
				(0.660)	(1.432)		
All	Monthly	Independent stores	T: all goods	-6.194***	2.530	5,613	292,219
				(0.702)	(2.013)		
			T: no caps	-6.108***	5.935***	3,277	174,937
				(0.874)	(1.769)		
			T: caps	-5.594***	2.972	5,154	267,947
				(0.742)	(2.344)		

Notes: This table presents the point estimates of the pass through. In particular, the specification pools the individual coefficients identified by the original equation (1) in the following way: $P_{i,s,r,t} = \alpha_{i,s,r} + \gamma_t + \delta\ W_t \cdot Treat_{i,s,r} + \beta\ W_i \cdot Post_i \cdot Treat_{i,s,r} + \epsilon_{it}$, where $P_{i,s,r,t}$ refers to the price of barcode i in store type s, in region r in a certain week/month-year denoted by t. α and γ are barcode and time fixed effects, respectively. $Treat$ equals one for those goods affected by the VAT change, while $Post$ identifies the period after the reform. $Window$ is a dummy that equals one for the time horizon comprised between the week/month prior to the reform and up to four months after its implementation (excluding the immediate week/month post reform). The table presents the β coefficient, which measures the change in prices relative to the pre-reform period. T and C refer to treated and control goods, respectively. The values that appear in the seventh column stand for *unique* barcodes (note that a given barcode may appear multiple times as it can be purchased in different store types s and regions r). Standard errors are clustered at the product–region level (category $\times$ region, approximately 160 clusters) and reported in parentheses; significance stars are based on the wild cluster bootstrap (Cameron–Gelbach–Miller, 999 replications) at the product–region level, the same inference procedure used for the confidence bands in the event-study figures. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table 4: VAT-Cut and VAT-Increase Pass-Through Estimated Separately by Reform

Goods	**Stores**	Price effect (percentage points) **VAT decrease** (rel. pre-cut)	**VAT increase** (rel. pre-increase)
Uncapped (Panel (b))	Chains	-12.74^{***}	$+18.07^{***}$
		(0.62)	(1.48)
	Independents	-6.11^{***}	$+13.51^{***}$
		(0.87)	(2.18)
Capped (Panel (c))	Chains	-14.91^{***}	$+11.46^{***}$
		(0.66)	(0.80)
	Independents	-5.59^{***}	$+10.09^{***}$
		(0.74)	(1.84)

Notes: This table reports the price effects of the VAT cut and of its re-introduction estimated as *separate* regressions, one per reform, and is the regression counterpart of the event-time panels in Figure 2 (Panel (b), uncapped goods; Panel (c), capped-category goods). Each reform is measured relative to its own pre-reform month: the VAT *decrease* relative to July 2019 (the month before the cut), averaged over the four months following the cut; the VAT *increase* relative to December 2019 (the month before the re-introduction), averaged over the five months following it. The increase coefficient therefore measures the rise from the post-cut price level (what Panels (b) and (c) plot), and differs from the re-introduction coefficient in Table 3, which is measured relative to the pre-cut baseline and thus reports the round-trip endpoint. The transition month of each reform is excluded. The full-pass-through benchmarks differ by reform—−17.4 p.p. for the cut and +21 p.p. for the increase—because a 21% rate is 21% of the net but only 17.4% of the gross price. The dependent variable is the barcode price normalized to 100 in the relevant pre-reform month; the specification absorbs barcode-by-region-by-store-type fixed effects and time fixed effects. Standard errors, clustered at the product–region level, are in parentheses; significance stars are based on the wild cluster bootstrap (Cameron–Gelbach–Miller, 999 replications) at the product–region level. ***, ** and * indicate significance at the 1%, 5% and 10% level.

A Anti-Profiteering Measures in Other Countries

This section documents the use of anti-profiteering policies in other developing countries during consumption tax reforms. While these measures vary in design and enforcement, they share the goal of ensuring that tax changes are reflected in consumer prices rather than captured as excess profit by firms. This non-exhaustive overview places the Argentine experience within a broader international landscape.

India. India enacted a comprehensive anti-profiteering framework as part of its Goods and Services Tax (GST) reform in 2017. Section 171 of the CGST Act requires that any reduction in tax rates or new input tax credits be passed on to consumers through commensurate price reductions. To enforce this, India established the National Anti-Profiteering Authority (NAA), which investigates complaints and can impose penalties. The law does not define "profiteering" precisely and leaves room for case-by-case interpretation by the Competition Commission of India. High-profile cases have targeted firms in sectors ranging from FMCGs to restaurants, where prices did not drop despite tax reductions.

Malaysia. Malaysia introduced the Price Control and Anti-Profiteering Act in 2011, ahead of its GST implementation in 2015 (OECD, 2024). The Act limited retailers' ability to increase their net profit margins beyond a regulated baseline during the GST transition. When GST was abolished in 2018 and replaced by a Sales and Services Tax (SST), the same framework was used to ensure prices declined accordingly. Enforcement was carried out by the Ministry of Domestic Trade, with fines and inspections used to deter violations (e.g., see here). This Act served as a central tool to manage price behavior in response to both tax increases and reductions.

Bangladesh. Bangladesh operates a Maximum Retail Price (MRP) regime that functions as a *de facto* anti-profiteering mechanism. All packaged goods must display an MRP that includes VAT, and retailers are prohibited from charging above it. When VAT structures were modified in 2025, the National Board of Revenue clarified that supermarkets could not add VAT on top of MRP, reinforcing that any tax changes must be absorbed within the existing price cap. In regulated sectors such as pharmaceuticals, MRPs are determined by authorities based on import prices and cost structures, ensuring that tax changes are passed on to consumers.

Indonesia. Indonesia uses a system of government-declared price ceilings known as Harga Eceran Tertinggi (HET) for essential goods like rice, sugar, and cooking oil. These ceilings cap final consumer prices, including taxes. During VAT increases (e.g., from 10% to 11% in 2022), the government adjusted HETs cautiously or temporarily absorbed part of the tax increase to avoid sharp retail price hikes. The HET mechanism thus attempts to limit the pass-through of tax increases and ensure that tax reductions or exemptions result in lower consumer prices, especially for subsidized goods.

Sri Lanka. Sri Lanka applies maximum retail price (MRP) controls on a range of essential goods, including food items, bottled water, and cooking gas. These MRPs are set by the Consumer Affairs Authority and are legally binding, limiting how much retailers can charge regardless of changes in input costs or taxes. When consumption tax changes occur (such as adjustments to VAT or import duties), the government can use MRP regulations to delay or limit price increases. Conversely, when tax reductions are enacted, MRPs may be revised downward or retailers are expected to adjust prices within the existing cap. MRPs are designed to curb opportunistic pricing and ensure that tax relief reaches consumers.

Philippines. The Philippines uses Suggested Retail Prices (SRPs) as a key consumer protection and price stabilization tool. The Department of Trade and Industry (DTI) regularly issues and updates SRPs for a list of basic food necessities and commodities. While not legally binding like MRPs, SRPs serve as reference prices and retailers are strongly discouraged from exceeding them, especially during periods of economic distress or fiscal reform. In the context of VAT changes, SRPs act as a soft ceiling, and enforcement agencies can conduct market inspections and penalize unjustified overpricing.

South Africa. When South Africa increased its VAT rate from 14% to 15% in 2018, the government did not enact a formal anti-profiteering law but relied on public warnings and oversight by the Competition Commission. Consumers were encouraged to report unjustified price increases, particularly on zero-rated goods. The government later expanded the list of VAT-exempt items to mitigate regressivity and expected retailers to fully pass through the tax relief. Although no legal caps were implemented, the policy environment emphasized voluntary compliance and reputational enforcement.

Other Examples. Several other L&MICs have used anti-profiteering approaches informally or through general price regulations:

- **Kenya:** During a temporary VAT cut in 2020, the government warned retailers to adjust prices accordingly. Enforcement was left to the competition authority and consumer pressure.
- **Nepal:** Introduced mandatory MRP labeling to combat unfair pricing during tax and cost shifts, facilitating consumer enforcement.
- **Pakistan:** Fixes retail prices in sensitive sectors such as pharmaceuticals and fuel, using general price controls to manage the pass-through of taxes.

While the institutional mechanisms vary, many developing countries have implemented anti-profiteering measures—either explicitly through laws and monitoring agencies or implicitly through statutory price caps like MRPs or HETs. These tools constrain how tax changes are passed through to consumers and help ensure that the intended effects of consumption tax reforms are realized in retail prices. The Argentine case is unique in targeting the *repeal* of a tax cut with formal price caps and provides rare empirical evidence on compliance, placing it among a limited set of documented international experiences with anti-profiteering enforcement when the VAT is reinstated.

B Macro Pricing Models and the Asymmetry-Hysteresis Puzzle

B.1 What Explains the Asymmetric Pass-Through and the Hysteresis

Our paper documents three puzzling facts. The first is that the prices of uncapped goods treated by the VAT cut exhibit asymmetric pass-through when the VAT cut is repealed. Below, we argue that asymmetric pass-through can be explained using standard macro pricing models. The second puzzling fact is that the prices of uncapped goods exhibit hysteresis, i.e., they do not converge to pre-VAT cut levels several periods after the repeal of the VAT cut. Similarly, the third puzzling finding is that the prices of capped goods do not converge to their pre-VAT cut levels once the cap enforcement expires. The second and third puzzles both show that prices exhibit hysteresis when responding to taxes or price regulations, which is a finding that has been documented in other settings as well (Benzarti et al. (2020)) and that standard macro pricing models cannot explain, as we discuss below.

While the canonical tax incidence model predicts symmetric pass-through of equal-sized tax changes (see Fullerton & Metcalf, 2002; Benzarti, 2025), this section shows that there are macro pricing models that can generate some degree of asymmetry. We provide intuition for these models here and expand the discussion in Appendix Section B.2 below.

There are two main classes of macro pricing models (see Nakamura & Steinsson, 2013): Calvo pricing models (Calvo, 1983) and menu cost models (Caplin & Spulber, 1987). Calvo models, which assume that opportunities to adjust prices occur at random, are unlikely to fit our data (as we discuss in more detail in Section B.2). Intuitively, these types of models predict sluggish price adjustments, but our evidence shows that prices respond immediately to VAT changes.

In contrast, menu cost models can generate asymmetric pass-through, whereby prices respond more to tax/cost increases than to decreases, in the presence of inflation. In this section, we provide intuition for the main mechanism through which these models generate asymmetric pass-through, and we describe them in more detail in Appendix Section B.2.

Assume that firms incur a menu cost every time they change prices. In this case, we show that firms decrease prices immediately after the VAT cut to account for the effect of the VAT cut, and also bundle some future price increases due to inflation, thus saving on menu costs. The price response to the VAT cut is therefore mitigated by the bundling of future price increases due to inflation.

Similarly, following the VAT increase, firms may increase prices above those of the control group, bundling future price increases due to inflation with the price increase due to the VAT increase and thus saving on menu costs. This results in aggregate nominal prices increasing in the treatment group and overshooting relative to the control group following the VAT increase. Treatment group prices remain stable for a few periods after the VAT increase (the number of which will depend on the relative magnitude of the menu costs and inflation) until the control group catches up with the treatment group. At this point, the two groups should have similar price levels.

While such a simple model can predict asymmetric pass-through and short-run price hysteresis, it is likely incomplete. In particular, the price difference between the control and treatment groups is supposed to converge to zero a few periods after the VAT increase according to the menu cost model predictions. In contrast, we observe that prices in the treatment group remain high several months after the VAT increase. For these reasons, additional features and assumptions or a different class of models, may be needed to match our empirical findings. To the best of our knowledge, there are no macro pricing models that predict medium- to long-run price hysteresis.

The same tension arises for the third puzzle. Once cap enforcement ceases, the menu-cost model above predicts that firms should re-adjust capped-goods prices toward their unconstrained optimal level, with the adjustment time again of order T^*. We observe instead that the cap-induced gap between capped and uncapped chain goods, approximately 9.5 percentage points at the VAT repeal in January 2020, persists essentially unchanged for at least nine months after cap enforcement was effectively suspended in March 2020 (Figure 3a). This is inconsistent with the same menu-cost mechanism that fails to predict the post-VAT-increase hysteresis of uncapped goods, suggesting that a different class of models is needed to rationalize the medium- to long-run hysteresis observed in our data, regardless of whether the underlying shock is a tax change or a regulatory one.

B.2 Macro literature

There are two main macro pricing models: Calvo models and menu cost models. We describe each below and discuss how well they fit our empirical findings of asymmetric pass-through and price hysteresis.

B.2.1 Calvo Model

Denote by p_t the log aggregate price level in the economy, and assume that each firm adjusts prices with a probability $1-\alpha$ in every period. Thus

$$p_t = (1-\alpha)p_{it}^* + \alpha p_{t-1} \tag{10}$$

where p_{it}^* is the log price of those firms that change prices in period t.

Assume that log nominal marginal costs mc_t are equal to log nominal wages w_t, i.e., firms produce using labor as the only variable input and a linear production technology. Denote by β the per-period discount factor and assume that the demand curve is given by $y_{it} - y_t = -\theta(p_{it} - p_t)$ where y_{it} denotes log demand for product i, y_t denotes log real aggregate output and p_{it} denotes the log real price of product i. One can show that:

$$p_{it}^* = (1-\alpha\beta)\sum_{j=0}^{\infty}(\alpha\beta)^j E_t mc_{t+j} \tag{11}$$

i.e., firms will set a price that will be a discounted average of the future marginal costs of the periods when prices will remain fixed.

Denote by m_t log nominal output, thus $m_t = y_t + p_t$ and assume that $m_t = \nu + m_{t-1} + \epsilon_t$, i.e., nominal output follows a random walk with drift. Assume that $\nu = 0$ for simplicity, i.e., there is no average growth in the economy. Assume that household utility is given by $c_t - L_t$ where c_t is log consumption and L_t is labor. This utility function implies that labor supply is perfectly inelastic, thus $c_t = w_t - p_t$. Since $mc_t = w_t$ and $m_t = y_t + p_t = c_t + p_t = (w_t - p_t) + p_t = w_t = mc_t$, and, because $m_t = m_{t-1} + \epsilon_t$, it follows that $E_t mc_{t+j} = E_t m_{t+j} = m_t$. Thus $p_{it}^* = (1-\alpha\beta)m_t \sum_{j=0}^{\infty}(\alpha\beta)^j = m_t$. Therefore:

$$p_t = (1-\alpha)m_t + \alpha p_{t-1} \tag{12}$$

This equation governs the evolution of log aggregate nominal prices, which respond sluggishly to aggregate shocks because they depend on previous prices. How sluggish prices are depends on α, i.e., the proportion of firms that keep prices unchanged in every period.

It is relatively straightforward to show that the Calvo pricing model does not fit our empirical findings well. Under a Calvo model, aggregate prices should adjust in a step-wise fashion. The intuition for this is easy to grasp: the Calvo model predicts that only a portion α of firms will adjust prices in every period, and so the impact on aggregate prices should be α times the optimal price in the first period, 2α in the second period etc. This should lead to a step-wise decrease in prices following the VAT cut and a step-wise increase in prices following the VAT increase. Because we observe immediate adjustments to prices for the VAT decrease and increase, Calvo pricing models are unlikely to match our empirical findings.

B.2.2 Menu Cost Models

The menu cost model we describe is based on Caplin & Spulber (1987). Using the same notation as above, assume that $m_t = y_t + p_t$ is always increasing (i.e., there is relatively high inflation). Contrary to the Calvo model, we assume that firms are able to change prices in every period (i.e., $\alpha = 0$) but they pay a fixed cost whenever they change prices. These assumptions imply that firms will follow a so-called sS policy: they change their relative prices by $S - s$ whenever relative prices drop below a trigger s. In this model, firms that change prices are not selected at random, as is the case in the Calvo model, but are selected to be the ones with the strongest incentive to change prices. As a consequence, these firms will change prices by a larger magnitude than in the Calvo model, leading to higher responses of aggregate price levels, which is often referred to as the "selection effect" (Golosov & Lucas (2007)).

The main intuition for why menu cost models may predict asymmetric pass-through rates of VAT changes to prices is that menu costs can cause firms to bundle several price changes together. This relies on the (standard) assumption that menu costs are a fixed cost, i.e., it is as costly to change prices by a little or by a lot. In the case of the VAT cut, firms bundle several future inflation-induced price increases with the VAT cut-induced price decrease. In the case of the VAT increase, firms may bundle the increase in price due to the VAT increase with future price increases due to inflation, thus saving on future menu costs in both cases.

We derive a very simple menu cost model to illustrate these patterns. Assume that the discount rate is set to zero, that there is constant inflation π at every period and that firms are price setters. Denote by p_t posted prices and p_t^* optimal prices in period t. Denote by $f_t(p_t - p_t^*)$ a loss function equal to the cost of having posted prices deviate from optimal prices and μ the menu cost, which is incurred whenever prices are changed (by any amount). A key assumption is that $f(\pi) < \mu$, i.e., for a given period, it is less costly to adjust prices to account for inflation than to pay the menu cost. It is also the case that there exists c such that $\sum_t^{t+c} f_t(\pi) > \mu$, i.e., that keeping prices fixed for more than a number of periods c, is more costly than incurring the menu cost μ.

Denote by $t = d$ the period of the VAT decrease and $t = i$ the period of the VAT increase. Firms will adjust prices in period t if $\sum_{t=i}^{T} f_i(p_{t-1} - p_i^*) > \mu$, i.e., if keeping period t to T posted prices equal to those of period $t - 1$ is more costly than incurring the menu cost μ.

Following the VAT decrease, firms will fully adjust prices if $f_d(p_{d-1} - p_d^*) > \mu$. They decrease prices to account for the VAT cut but also bundle some of the future price increases due to inflation. Therefore, they decrease prices by the amount needed to account for the VAT decrease minus πT^*, where T^* is given by $\sum_{t=i}^{T^*} f_i(p_{t-1} - p_i^*) = \mu$. And they hold prices fixed for T^* periods after that.

Inflation acts in an asymmetric way on incentives to change prices for the VAT increase and decrease, which is what causes the asymmetry in price adjustments. Following the VAT increase, firms will adjust prices to account for the VAT increase and also bundle some future price increases due to inflation. The number of periods that are bundled into the price increase due to the VAT increase is also given by T^* such that $\sum_{t=i}^{T^*} f_i(p_{t-1} - p_i^*) = \mu$. Therefore, firms are increasing prices by the amount of VAT increase pass-through and also by πT^*. Firms pass through the VAT increase and future price adjustments due to inflation, in order to save on menu costs. Therefore, prices will over-shoot above those of the control group by πT^* and will remain unchanged for a number of periods T^*. This is the over-shooting we observe in our empirical evidence, and the hysteresis should last T^* periods.

While this simple model predicts that prices will respond asymmetrically to the VAT decrease and increase, and that there will be some degree of price hysteresis following the VAT increase, there are several predictions that do not match our empirical findings.

The first one is that the model predicts step-wise adjustments in prices as a response to inflation: outside of the VAT change periods, firms will also bundle future inflation changes into one price change. This predicts that prices adjust every T^* periods rather than every period. This step-wise function can easily be smoothed by assuming that firms face a distribution of idiosyncratic shocks (in addition to the two VAT changes), leading a set of firms to increase prices in every period.

The second prediction that does not match our empirical findings is that prices in the treatment group should converge to those of the control group after T^* periods. This prediction is clearly inconsistent with our findings, which show that prices in the treatment group remain higher than those of the control group for at least a year. T^* equal to one year would imply unrealistically large menu costs. There is no "easy fix" for this assumption, and to our knowledge, more sophisticated models cannot account for this degree of hysteresis. Therefore, this finding may require a new class of models, such as coordination failure-based models (Cooper & John (1988) and Ball & Romer (1991)), which generate significantly more price rigidity.

B.3 Micro literature (Evasion, Competition and Pricing Strategies)

While we do not know with certainty what could be driving the differences in pass-through rates of the VAT cut in independent versus chain supermarkets, our understanding of the political environment at the time of the VAT cut suggests that this might be due to two complementary facts: (1) the government exerted significant political pressure on supermarkets to try and pass through as much of the VAT cut as possible. Government officials even had meetings with the executives of the four largest supermarket chains (Carrefour, Walmart, Jumbo, La Anonima) to try and have them cut prices as much as possible following the VAT cut. For this reason, they may have been more receptive to the political pressure; and (2) the government's price monitoring system (which is not the dataset we use in our analysis) mostly collects data from supermarket chains. Hence, since independent supermarkets know that the government cannot easily observe the prices they charge, they can more easily avoid cutting prices without incurring much political fallback.

Here, we address three alternative explanations for the muted response to the VAT cut in independent supermarkets (which we describe below in more details): (1) a higher propensity to evade VATs in independent supermarkets, (2) pricing strategy differences across store types, and, (3) different levels of competition across store types.

B.3.1 Evasion

A possible explanation for the fact that chain and independent supermarkets respond differently to the VAT cut is the fact that chain supermarkets are likely to operate more formally than independent ones, thus more likely to issue receipts and charge the VAT. Conversely, independent supermarkets might evade the VAT by relying more on cash transactions. If that is the case, VAT changes would have dampened effects on prices in independent supermarkets, leading to muted pass-through rates of both VAT cuts and VAT increases. Bachas et al. (2023) provide very compelling evidence in support of this explanation in low-income countries around the world. They show that consumers at so called "traditional stores", which are small mom-and-pop shops, tend to bear less of the VAT than consumers at modern stores. There are two important distinctions between our setting and the one Bachas et al. (2023) consider. First, Argentina is a more advanced economy than the set of countries that Bachas et al. (2023) consider. For example, the share of informal consumption, defined as transactions occurring in stores that are not registered for the VAT, is only 3% in Argentina.[24] This share is substantially higher in the countries considered by Bachas et al. (2023) and averages more than 30%. Second, the independent supermarkets we consider would actually be classified as modern stores by Bachas et al. (2023) as opposed to traditional stores, which are mom-and-pop shops and are excluded from our analysis. Nevertheless, this explanation could be at play in our setting. While gathering evidence of tax evasion is impossible using our data, we provide a model that shows such pass-through effects would exist if evasion is more prevalent in independent supermarkets.

The following model builds on Kopczuk et al. (2016). The equilibrium condition is given by $D(p) = S(p,t)$, where D is demand, S is supply, p is price and t is a per unit tax remitted by the supply side. Denote by: $c(q)$ the variable cost of producing q units of the good, F the fixed costs of production, $\Phi(e)$, the cost of evasion, where $0 \leq e \leq 1$ is the quantity of evasion.

Firm profit is given by $\Pi(q,e) = p.q - c(q) + t.e - \Phi(e) - F$

Optimal evasion is simply given by: $t = \Phi'(e)$: the higher the tax rate, the more evasion the company engages in. This equation implicitly defines the optimal evasion rate: $e^*(t)$.

The production decision is given by the first order condition: $p = c'(q)$, which implicitly determines the optimal quantity: $q^*(p)$.

Two additional conditions are needed to close the model: (1) a zero profit condition: $\Pi = \Pi^v(p) - (F - R(t)) = 0$, where $\Pi^v(\cdot)$ is operating profits (no fixed costs) and $R(\cdot)$ is the revenue from tax evasion; and (2) a free entry condition: $Nq^*(p) = Q(p+t)$, where $Q(\cdot)$ is total demand and N the number of firms.

The first order condition is given by $\frac{\partial \Pi^v}{\partial p}\frac{dp}{dt} + R'(t) = 0$. Using the envelope theorem for $\Pi^v(\cdot)$ and $R(\cdot)$, we get the following pass through formula:

$$\frac{dp}{dt} = \frac{e^*(t)}{q^*(p)}$$

If the tax rate increases, the net of tax price received by producers falls by $\frac{e^*}{q^*}$, hence consumer price increases by $1 - \frac{e^*}{q^*}$. If tax evasion is $e = 0$, then the full incidence is on consumers, which is due to the fact that supply is perfectly elastic given the free entry and zero profit conditions. If tax evasion is indeed higher in independent supermarkets, then we would expect a muted pass-through of the VAT cut, even with identical supply and demand elasticities, relative to supermarket chains, which would be consistent with our empirical evidence.

[24] Source: See page 31, Table 2.4 in https://www.indec.gob.ar/ftp/cuadros/sociedad/engho_2017_2018_resultados_preliminares.pdf

B.3.2 Competition

Differences in pass-through may be due to different levels of competition in chain and independent supermarkets.[25] For instance, it could be that independent stores are located in more isolated places. Genakos & Pagliero (2022) show how pass-through varies with competition in isolated oligopolistic markets in Greece. The setting in this paper is different from ours for two reasons. First, they focus on a particular and specific market (gasoline). Second, they look at a specific geographical setting (islands).

We show in Appendix Figure E.31 that competition indeed affects pass-through rates, in a minor way. This figure pools chain and independent supermarkets and breaks down pass-through rate estimates for goods at the barcode level that are sold in both types of supermarkets versus goods that are sold in either one of them but not both. Presumably, goods that are sold in both chain and independent supermarkets will be more competitive, thus affecting pass-through rates (Weyl & Fabinger (2013)). This is indeed what the figure shows: the pass-through rate for goods that are present in both supermarket and independent chains is 12 percentage points, while that of goods that are only present in one of them is 9 percentage points. This suggests that competition is likely driving some of the differences in pass-through rates.

B.3.3 Different Pricing Strategies

Another explanation could be that chain and independent supermarkets follow different pricing strategies. DellaVigna & Gentzkow (2019) and Harju et al. (2018a) provide compelling evidence of such behavior in different settings. DellaVigna & Gentzkow (2019) provide evidence that national chains respond differently to local shocks compared to local supermarkets in the US. While, Harju et al. (2018a) estimate the incidence of a large VAT cut on chain versus independent restaurants in Finland and find that chain restaurants pass through 100% of the VAT cut in the short run, while independent restaurants pass through 0% of the VAT cut. Notably, these patterns revert in the medium run, whereby chain restaurants start raising their prices leading their pass-through rates to converge to those of independent restaurants. At first glance, this behavior appears consistent with the findings from our paper. However, there are two main differences. First, the shock we analyze is not a local shock, and is instead a national policy, which is inconsistent with the explanation from DellaVigna & Gentzkow (2019). Second, we do not observe a convergence in pass-through rates in chain and independent supermarkets, as shown in Harju et al. (2018a).

We show, for example, that chain and independent supermarkets respond very similarly to other economic shocks when there is no government interference. In particular, we provide evidence that chain and independent supermarkets display similar pricing behavior when responding to changes in currency value, which directly affect prices. Indeed, the Argentine peso experienced a large and sudden devaluation on August 30th, 2018, causing a 24% increase in the exchange rate of the peso against the US dollar, which is plotted in Appendix Figure E.1. Moreover, the cumulative depreciation between January and September 2018 was 100% and the exchange rate remained relatively stable thereafter. As a consequence, supermarkets had to adjust their prices. In Appendix Figure E.32, we plot the distribution of price changes in supermarket chains in the upper panel and in independent stores in the bottom panel as a response to the large and sudden devaluation

[25] Another market structure explanation could be that there are differences in vertical integration in chain and independent supermarkets (See Bajo-Buenestado & Borrella-Mas (2022), Fuest et al. (2024), Gopinath & Itskhoki (2010) and Hellerstein & Villas-Boas (2010). Such differences could be due, in theory, to markups attenuating the effect of changes in marginal costs (see Hong & Li (2017)).

of the peso. The red distribution plots the differences in prices between September 2018 and July 2018, effectively capturing the pass-through of the devaluation to prices. As a placebo, we also plot, in gray, the difference in prices between July and May. The distribution of price changes of the devaluation is very similar for chain and independent stores, suggesting that, when there is no political pressure exerted by the government, chain and independent supermarkets behave very similarly.

In addition, Appendix Figure E.17b reports the average effect of the depreciation on the prices of goods that were later subject to the VAT cut relative to those that were not. In contrast to the sharp differential response of chain and independent supermarkets to the VAT cut, the figure suggests that supermarkets responded similarly to the currency depreciation shock.[26] Overall, our evidence suggests that pricing strategies that are inherently different for chain and independent supermarkets are unlikely to explain the difference in pass-through rates.

C Effect of the VAT Cut on Government Revenue

To assess the macro revenue consequences of the VAT cut, we further use semi-aggregate administrative data from the tax administration's Statistical Yearbooks and apply a macro diff-in-diffs approach. These tax tabulations (derived from VAT returns F.731 and F.2002) contain detailed 3-digit sector-level information by year, including the number of tax returns per sector, input VAT paid on purchases, output VAT charged on sales, and the volume of taxable and non-taxable sales. Appendix Table A5 presents an excerpt for 2019.

We compiled a panel covering nine retail sub-sectors from 2015 to 2023 (see Appendix Table A5 and here for a full list of sub-sectors in the data). For each sector, we construct the net VAT liability by subtracting the total input VAT from the total output VAT. This is our main outcome in the diff-in-diffs analysis below.

Two remarkable features of the data are worth highlighting. First, among the nine sectors, we observe grocery stores separately (sector 471 in Appendix Table A5). This comprises both chain supermarkets and independent grocery stores affected by the policy, allowing us to accurately isolate tax revenue effects arising from the treated sector. Second, firms selling zero-rated foodstuffs were required to report these sales under "non-taxable and exempt operations," a separate field in the tax return recorded in the yearbooks (column 3 of Appendix Table A5). This distinction enables us to verify the mechanism at play: any decline in VAT revenue should correspond to an increase in reported non-taxable sales.

Using these data, we estimate a diff-in-diffs regression comparing grocery stores (sub-sector 471) with the remaining retailers. Note that this mirrors the comparison we would conduct if we had firm-level tax microdata, albeit using sector-aggregated volumes. The main drawbacks of our approach are reduced statistical power (due to sector-level rather than firm-level data) and limited control flexibility (e.g., we cannot include firm-fixed effects). Our outcomes include non-taxable sales, total sales, and net VAT revenue. We exclude two subsectors: (i) subsector 472 (Retail sale

[26] Figure E.33 extends the analysis to encompass the full period from 2018 to 2020, using July 2019 as the reference month. The fluctuations observed in 2018 are primarily attributed to the depreciation of the peso (Appendix Figure E.17a). This event appears to have had a similar impact on both chain and independent stores. Furthermore, during the initial eight months of 2019, a period characterized by a fairly stable exchange rate, both treated and control products exhibit parallel trends in these stores. This evidence mitigates the concern that the observed differences in how chain and independent stores responded during the VAT holiday were due to the potential varying impact of the peso depreciation on their profit margins.

of food, beverages, and tobacco products in specialized stores), which contains a mix of treated and mostly untreated firms, and (ii) subsector 478 (Retail sales in mobile stalls and markets), a small, largely informal sector with about 450 returns that slightly distort the trends.

Two key results emerge from this exercise. First, Figure E.34 shows a sharp, statistically significant temporary increase in non-taxable sales reported in 2019 (blue line); non-taxable sales were 2.4% of total grocery-store sales in 2018. As mentioned before, this reflects a mechanical response: firms reclassified the (now) zero-rated sales in the designated tax return field. Notably, the macro data capture this pattern clearly. The figure also presents the diff-in-diffs effect on total sales (red line). The null effect in 2019 confirms that firms merely shifted sales from taxable to non-taxable categories, with no significant general equilibrium effects. The positive sales effect in 2020 reflects the pandemic, during which most retail sub-sectors saw declines while grocery stores remained open. Reassuringly, both outcomes exhibit parallel pre-trends before the reform, strengthening the credibility of our exercise.

Second, Figure E.29 reveals a substantial 30% decline in net VAT revenue for grocery stores in 2019 relative to other retailers. The top panel displays log levels, while the bottom panel shows diff-in-diffs coefficients. The net VAT rebound in 2020 aligns with the pandemic-driven patterns seen in Figure E.34.

Exact fiscal implications from official tax expenditure reports. Are these effects economically significant? Our analysis suggests they are. According to our estimates, the federal tax agency forwent about one-third of VAT revenue from grocery retailers, a major sector of the economy, during the period of the policy. Remarkably, this fiscal impact is independently confirmed by official tax expenditure reports, which systematically track revenue forgone due to exemptions, deductions, and preferential rates across all major taxes (see here). Argentina's tax expenditure system is unusually detailed by international standards: since 2001, it has reported annual estimates disaggregated by tax type and policy.

What makes the 2019 report particularly valuable is that it included a dedicated line quantifying the fiscal cost of the four-month VAT cut on basic food items. This figure, 0.07% of GDP, was not based on assumptions or modeling, but rather derived directly from administrative data: under the reform, firms were required to report sales of newly zero-rated goods in a specific field of the VAT return ("non-taxable and exempt operations"), which enabled the government to measure the revenue impact with unusual precision.

For context, Argentina's flagship conditional cash transfer program (AUH) costs approximately 0.4% to 0.6% of GDP annually. Annualizing the VAT holiday's cost yields 0.21% of GDP—about half the cost of the AUH. This comparison underscores that the temporary VAT cut entailed a nontrivial fiscal cost, especially given its limited pass-through to consumer prices.

D Consumer Welfare Model Extensions

D.1 Changes in Expenditure Shares

In this Section, we show that changes in expenditure shares are second-order. Let $\alpha_{gh}(p)$ denote the expenditure share. At the initial prices p_0, the expenditure share $\alpha_{gh}(p_0)$ is known. When prices change due to the VAT reform, the expenditure share may shift. But this shift can be written as a Taylor expansion around p_0

$$\alpha_{gh}(p) = \alpha_{gh}(p_0) + \frac{\partial \alpha_{gh}}{\partial p}\Delta p + O(\Delta p^2). \tag{13}$$

Thus, the initial expenditure shares are first order while changes in expenditure shares are second order. This is because the first derivative of the indirect utility function depends on the initial shares, and the change in consumer surplus comes from the initial level of expenditure shares, not their change. Any changes in α_{gh} caused by price changes will contribute to higher-order terms, which are second-order effects and are thus small.

D.2 Non-CES Utility Functions

Assuming CES preferences primarily simplifies demand derivation. However, the key expression (the impact of VAT changes as a function of expenditure shares and price changes) follows more generally for any continuous utility function that represents locally non-satiated, convex, and homothetic preferences. Under homotheticity, we can write the indirect utility function as real income:

$$V(p, w) = \frac{w}{P(p)},$$

where $P(p) \equiv \sum_g p_g h_g(p, 1)$ from the expenditure minimization problem (noting that $e(p, u) = P(p) \cdot u$). Using total differentiation, Shephard's Lemma, and the fact that the demand function is homogeneous of degree 1, we get:

$$d \ln V = d \ln w - d \ln P(p), \tag{14}$$

$$d \ln V = d \ln w - \sum_g \left(\frac{h_g(p, 1)p_g}{P(p)} d \ln p_g + \frac{p_g}{P(p)} \frac{\partial h_g(p, 1)}{\partial p} \cdot dp \right) \tag{15}$$

Defining expenditure shares as $\alpha_g(p) \equiv \frac{p_g h_g(p,1)}{\sum_g p_g h_g(p,1)}$ yields

$$d \ln V = d \ln w - \sum_g \alpha_g \cdot d \ln p_g + (\text{second-order terms}) \tag{16}$$

From the envelope theorem, changes in demand caused by price changes have no first-order effect on consumers' *expenditure* as long as we are located at an optimum in the expenditure minimization problem:

$$d \ln V = d \ln w - \sum_g \alpha_g \cdot d \ln p_g, \tag{17}$$

which can be interpreted as a first-order approximation. Note that with non-homothetic preferences, a VAT cut might have different effects across income levels because spending patterns shift as people get richer or poorer. Homothetic preferences simplify the analysis because expenditure shares depend only on prices and not on income. They allow us to express the expenditure function as $e(p, u) = P(p) \cdot u$, where $P(p)$ is homogeneous of degree one with respect to prices. In practice, we assume homothetic preferences within income deciles, retaining the tractability of homotheticity while allowing for heterogeneous expenditure shares across the income distribution (this is similar to Faber & Fally, 2022, who define five consumer income groups in terms of quintiles of the U.S. income distribution). Hence, the standard first-order formula still applies within each decile. This approach captures most of the relevant heterogeneity while avoiding a full structural model (since non-homothetic preferences would introduce second-order effects, such as income elasticities, that affect demand). Note that our approach is less accurate if (i) the VAT change induces major shifts in spending patterns across deciles (e.g., if lower-income households dramatically shift consumption toward or away from certain products), or (ii) there are strong income elasticities that drive substitution between necessities and luxuries (non-homothetic preferences would matter even within deciles, requiring a more flexible approach). We regard this as a reasonable approximation, given that the reform targeted basic food necessities with relatively stable budget shares across the income distribution.

D.3 Incorporating Income Shocks for Shareholders

This appendix derives the income-shock extension summarized in Section 5.6.1.

We extend the model to account for profits by incorporating income effects. We allow $d\ln w_h$ to vary with firms' profits, which are affected by the reform. This is particularly relevant if consumers hold equity in firms whose profits change due to VAT adjustments. Starting from our general expression (5):

$$d\ln V_h = d\ln w_h - \sum_g \alpha_{gh} d\ln P_{gh}$$

Suppose household income includes labor income and capital income, $w_h = w_h^L + w_h^K$. For consumers who are also firm owners (shareholders), their capital income depends on firm profits:

$$w_h^K = \sum_f \lambda_{hf}\pi_f, \tag{18}$$

where λ_{hf} is the share of firm f's profits owned by household h and π_f is the profit of firm f. Thus, changes in household income can be written as:

$$d\ln w_h = s_h^L \cdot d\ln w_h^L + \sum_f s_h^K \cdot d\ln \pi_f, \tag{19}$$

where $s_h^L = w_h^L/w_h$ and $s_h^K = \lambda_{hf}\pi_f/w_h$ are the income shares. Assuming no direct wage effects ($d\ln w_h^L = 0$) yields:

$$d\ln V_h = \sum_f s_h^K d\ln \pi_f - \sum_g \alpha_{gh} d\ln P_{gh}. \tag{20}$$

This shows that indirect utility changes depend not only on the price effects but also on how firm profits change and how much of those profits are owned by a given household. Using the profit function:

$$\pi_f = \sum_g (p_g^f - \mu_g) q_g,$$

where p_g^f is the *tax-exclusive* price of product g and μ_g is the marginal cost. Assuming a constant markup, $\mu_g = \frac{\sigma_g - 1}{\sigma_g} p_g^f$, then:

$$\pi_f = \sum_g \frac{p_g^f q_g}{\sigma_g},$$

$$d \ln \pi_f = \sum_g \vartheta_g (d \ln p_g^f + d \ln q_g), \tag{21}$$

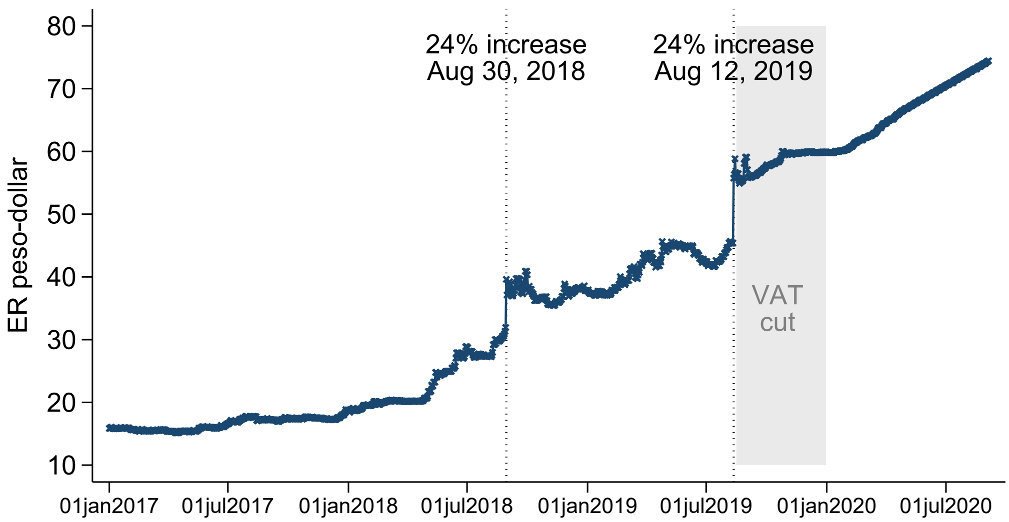
where $\vartheta_g = \frac{p_g^f q_g}{\sum_r p_r^f q_r}$ is the initial revenue share of product g in the firm's total revenue.

If a household holds a lot of equity (λ_{hf} is large), their income can increase if supermarkets benefit from the reform. Conversely, if firms suffer losses, shareholder households may face income declines, reducing their indirect utility beyond just the price effects. This matters for heterogeneity—higher-income households with more capital may experience different effects than wage-earning households.

Supplementary Materials for: "Can VAT Cuts and Anti-Profiteering Measures Dampen the Effects of Food Price Inflation?"

E Additional figures and tables

Figure E.1: Exchange rate (pesos per dollar)



Source: BCRA, Tipo de Cambio de Referencia - Comunicación "A" 3500 (Mayorista).

Figure E.2: Salience of the monitoring app “Precios Claros”

Notes: These pictures illustrate the salience of the monitoring app “Precios Claros” launched by the government in 2016. The top left panel shows the front page of one of the main newspapers in Argentina informing that the government launched a monitoring system for consumers to control prices in supermarkets. The bottom left panel shows the official webpage where consumers can consult any price in any store in Argentina. The bottom right panel shows an example of how the query looks like. The top right panel shows that the same information can be accessed through an app.

Figure E.3: Enforcement of the VAT increase and caps


Clarín

Suscribite por $1020

Hola, Dario
SIN SUSCRIPCIÓN

En vivo | Javier Milei y todas sus medidas | Dólar blue: la cotización este 27 de mayo | Juicio por la muerte de Diego Maradona | River vs Universitario

Economía - Economía - Economía

El Gobierno asegura que controlará "online" que se cumpla el nuevo acuerdo por los precios de los alimentos

Será a través de la Secretaría de Comercio, según confiaron a Clarín fuentes oficiales.

El ministro de Producción, Matías Kulfas, estuvo a cargo del acuerdo por el IVA de los alimentos.

EDUARDO PALADINI

0

01/01/2020 11:26 / Actualizado al 05/01/2020 17:22

Notes: This image shows media coverage of the reintroduction of the VAT and the government's anti-profiteering measures. The screenshot is from a Clarín news article published on January 1, 2020, titled *"The government assures that it will control 'online' that the new food price agreement is fulfilled."* (link). The article explains that the Ministry of Commerce would use its electronic monitoring system to track compliance in real time. A public official is quoted saying: *"Supermarkets report their prices online to the Ministry of Commerce. The database is updated as soon as they upload the price lists, and we can see it. The sector already showed goodwill by working with us until December 31 and committed to absorbing two-thirds of the impact. But obviously, we'll be monitoring them."* The image features Matías Kulfas, then Minister of Productive Development, who led the implementation of these measures. A separate article from Clarín, also published on January 1, 2020, confirms that independent grocery stores were not covered by the anti-profiteering policy (link).

Figure E.4: Media coverage of the VAT cut and subsequent hike

(a) **Media coverage of the VAT cut**

Clarín

Viernes 16.8.2019

Spot

En la piel de Carlos Tevez

Balthazar Murillo, un chico de 15 años, interpreta al crack de Boca en la serie "Apache", que se estrena hoy.

Soldano, la nueva apuesta de gol en Boca

"Mi desafío más grande", afirma. Debuta el domingo.

Tema del día • Nuevas medidas económicas

Eliminan IVA en alimentos, congelan cuotas UVA y habría cambios de Gabinete

Para Alberto F. el dólar a $ 60 está bien y no hay razón para que aumente

Del Editor

Ricardo Kirschbaum

El diálogo y un atisbo de tregua

Diosdado Cabello: "Que no vaya a creer que lo están eligiendo porque es él"

En busca de la mística perdida

Lavagna suspendió la campaña y pidió que lo imiten

Nuevo ataque del hacker que filtró datos de la Federal

TEVEZ LLEGA A NETFLIX

VILLA LA ANGOSTURA

EMERGENCIA AÉREA

LA NACION

Viernes 16 de agosto de 2019

Macri eliminó el IVA de 14 alimentos y congeló las cuotas de los créditos UVA

ECONOMÍA. Alcanza a productos de la canasta básica, como leche, pan, yerba, harina y arroz; regirá hasta diciembre, igual que la suspensión del aumento de los préstamos hipotecarios

Dólar: cayó por primera vez desde las PASO

Presión militar china a Hong Kong

Permitirán estacionar junto a algunas bicisendas

Filtraron archivos de la Federal sobre Chocobar

Ejecutaron a tres hombres en un búnker de drogas

Devaluación. Cómo se protegen los clubes

(b) **Media coverage of the VAT increase along the anti-profiteering caps**

Clarín — Jueves 2.1.2020 — BUENOS AIRES, ARGENTINA

Un toque de atención para la solución argentina de los problemas argentinos

Series 2020 Lo que llega en enero. Las ficciones más esperadas, y las más nominadas, para ver en cable y en las distintas plataformas. P.38

Patrick Stewart. Protagoniza Star Trek: Picard

Sin heridos por pirotecnia en el Instituto del Quemado. En la noche de Fin de Año y por primera vez desde 1982. P.24

A partir de hoy, con la excepción de la leche

Aumentan hasta 10,5% los productos de la canasta básica por la vuelta del IVA

El Gobierno acordó con proveedores y supermercados no trasladar al consumidor la alícuota de 21% que se volverá a aplicar desde hoy en productos básicos. Será en trece grupos de comestibles, que llegan a 400 alimentos. Experimentarán un aumento promedio de 7% en pan lactal, yerba, yogur, arroz, azúcar, huevos y polenta. Habrá picos de 9% en aceites y 10,5% en pan rallado. El pan artesanal y de panadería subirá 5 por ciento. La Secretaría de Comercio controlará online que las empresas cumplan lo pactado. Se relanzará además Precios Cuidados. P.6

A 5 AÑOS DE LA MUERTE DEL FISCAL

Nisman: Alberto dice ahora que no hay pruebas para afirmar que fue asesinado

El Presidente también criticó la pericia de Gendarmería, que ya propuso revisar la ministra de Seguridad. Fue al ser consultado por Clarín sobre lo que declaró en 2017, en un documental sobre el caso que estrenó ayer Netflix: "Dudo que se haya suicidado", dijo entonces. P.14

MAXI FAILLA

Por la Rambla. Al mal tiempo, buena cara. En Mar del Plata los turistas igual se animan al paseo.

El Personaje — CARLOS GHOSN

El ejecutivo que se fugó adentro de un estuche

Acusado de múltiples delitos financieros, el ex presidente de Renault-Nissan se escapó de Japón escondido en la caja de un contrabajo. P.18

LA PLATA

Matan a un jubilado frente a su mujer en un intento de robo

Jorge Pechiari tenía 77 años. Los ladrones dijeron que pedían cartón. P.26

MAR DEL PLATA, CARILÓ Y PINAMAR

Con mal clima y buen panorama la Costa arrancó su temporada

En los principales destinos de playa la ocupación es mayor a la del año pasado y se espera que siga así, en parte por el efecto del "dólar tarjeta", que desalentó algunos viajes a Brasil y Uruguay. En Mar del Plata, las reservas de alojamiento ya trepan al 75% y en Pinamar superan el 85%. Las estadías son en promedio de entre 4 y 5 noches. Lo que todavía no apareció es el sol ni el calor. En los últimos días, el tiempo estuvo lluvioso y con viento, y el pronóstico para lo que resta de la semana no es alentador, aunque según el Servicio Meteorológico el sábado mejoraría. TEMA DEL DÍA P.3

Del Editor — Pablo Vaca

El optimismo nuestro de cada año nuevo P.2

INFIERNO EN AUSTRALIA MILES DE ATRAPADOS ENTRE EL FUEGO Y EL MAR — EL MUNDO | Página 5

AÑO OLÍMPICO Tokio organizará los Juegos más tecnológicos de la historia; qué chances tienen los deportistas argentinos — DEPORTES

VIH-SIDA Avanzan los estudios en nuevas estrategias para el control y la prevención del mal — CIENCIA & SALUD | Página 21

LA NACION — Jueves 2 de enero de 2020

Rigen aumentos de entre 7 y 10,5% en la canasta básica y prometen más controles

CONSUMO. La Casa Rosada no prorrogó el IVA del 0% y acordó morigerar el traslado a precios con supermercados y la industria, que absorberán parte del costo; harán un monitoreo de las alzas

Francisco Jueguen, LA NACION

Los productos de la canasta básica de alimentos sufrirán desde hoy aumentos de entre 7% y 10,5%, un tercio de lo que podrían haber subido si volvía a trasladarse el 21% del IVA al valor final en las góndolas, y algo menos para aquellos que estuvieran gravados en un 10,5%. La leche fluida será el único alimento que no sufrirá ajustes.

Luego de reuniones que duraron casi siete horas, el Gobierno cerró un acuerdo con supermercados y la industria alimentaria para amortiguar el impacto de la decisión de no prorrogar la reducción del IVA al 0% para una canasta básica, que vencía anteayer.

En el Ministerio de Desarrollo Productivo afirmaron a LA NACION que el cumplimiento del acuerdo será monitoreado de manera online. Indicaron, a su vez, que el nuevo esquema que pretenden para el consumo masivo estará vigente durante este mes para el público. Continúa en la página 16

KICILLOF, DÍA CLAVE. **El gobernador bonaerense, Axel Kicillof, tendrá hoy un día clave para destrabar la postergada reforma impositiva. Se reunirá con intendentes y legisladores.** Página 6

Cuatriciclos, con más restricciones

PINAMAR (De un enviado especial).– Es uno de los grandes problemas de cada verano. Este año, los controles de cuatriciclos y de UTV –una suerte de buggy– serán más estrictos en la costa: por primera vez, los conductores deberán acreditar la aprobación de un curso especial para su manejo. A esta medida se suman las de la temporada pasada, como la habilitación de unas 30 calles y de una playa –La Frontera– para circular (foto), además de la obligatoriedad del uso de casco, antiparras y un banderín de dos metros de altura para ser advertido en los médanos. Página 24

EL ESCENARIO — **El vaivén con las naftas, un símbolo del nuevo gobierno** — Carlos Pagni

Cambio de Fernández sobre el caso Nisman — NETFLIX. En un documental grabado en 2017 dudó de un suicidio; ayer dijo que no hay pruebas de un asesinato. Página 14

Soja: el 53% por hectárea quedaría para el Estado — INFORME. Advierten que los productores sufrirían pérdidas por la sobrecarga tributaria. Página 17

Alarma por la escalada de tensión entre EE.UU. e Irán — MEDIO ORIENTE. Trump amenazó al régimen tras un asalto de milicias a la embajada en Bagdad. Página 2

Pirotecnia: por segundo año seguido, bajó el número de heridos en la ciudad — FESTEJOS. Lo atribuyen a las campañas de prevención en redes sociales. Página 22

Además — EL MUNDO: El Papa pidió perdón. Se disculpó por haber "perdido la paciencia" con una mujer que lo tironeó del brazo. Página 4 — CULTURA: Los 100 años de Asimov. El mundo parece atravesado por las visiones del genio, sinónimo de ciencia ficción. Página 20

Notes: These pictures show the media coverage of the VAT removal (panel a) and VAT reintroduction (panel b) in the two main newspapers of Argentina. The left panels correspond to "Clarin" newspaper and the right panels to "La Nacion" newspaper. In both newspapers, the main news of the day discusses the VAT cut (panel a) and the regulated VAT reintroduction with capped price increases (panel b).

Figure E.5: Salience of the VAT holiday

Notes: These pictures illustrate the salience of the VAT holiday in supermarkets. The top left panel shows a banner displayed at the entrance of a store informing that 13 products now face a temporary 0% VAT rate. The bottom left panel shows a large banner inside a store informing that more than 1,900 products (within the 13 treated categories) now face a temporary 0% VAT rate. The two right panels show mandatory tags that supermarkets had to display next to treated products.

Figure E.6: Geographic variables in the data

(a) **Supermarket chains**

GBA	CAPITAL FEDERAL		Capital Federal
	PERIFERIA		Suburbio Norte, Suburbio Sur, Suburbio Oeste
INTERIOR	BS. AS. RESTO		Pcia Bs As NO incluídas en la periferia
	CORDOBA		Pcia Córdoba
	ANDINA	CUYO	Pcias Mendoza, San Juan, San Luis
		NOA	Pcias Tucumán, Catamarca, Jujuy, La Rioja, Salta, Santiago del Estero
	LITORAL	LIT NORTE	Pcias Corrientes, Chaco, Formosa, Misiones
		LIT SUR	Pcia Santa Fe y Entre Ríos
	SUR		Pcias La Pampa, Neuquen, Río Negro
	AUSTRAL		Pcias Chubut, Santa Cruz, Tierra del Fuego

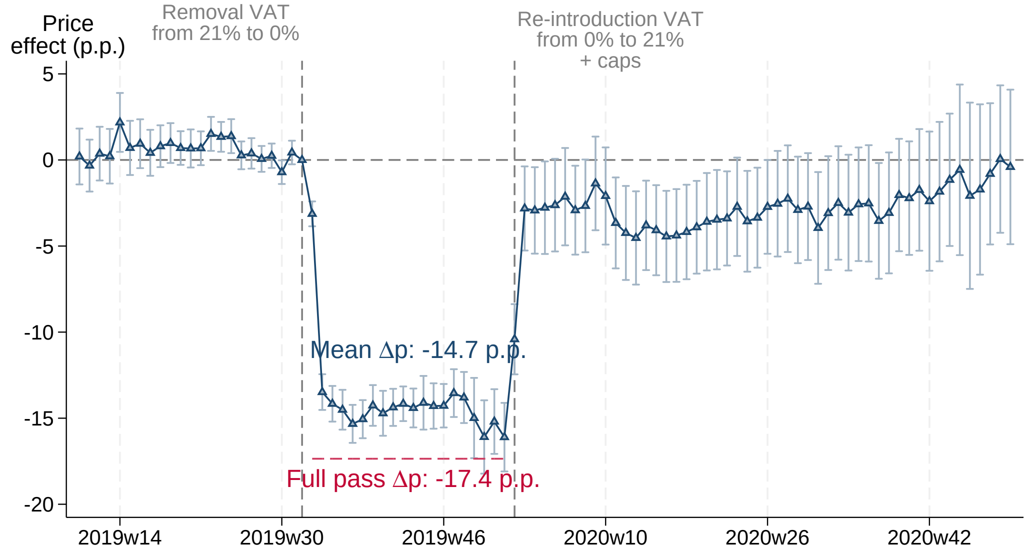
(b) **Independent supermarkets**

GBA	GBA	Capital Federal, Suburbio Norte, Suburbio Sur, Suburbio Oeste
INTERIOR	BS. AS. RESTO + SUR	Pcia Bs As NO incluídas en la periferia + Pcias La Pampa, Neuquen, Río Negro, Chubut, Santa Cruz, Tierra del Fuego
	CORDOBA	Pcia Córdoba
	ANDINA	Pcias Mendoza, San Juan, San Luis, Tucumán, Catamarca, Jujuy, La Rioja, Salta, Santiago del Estero
	LITORAL	Pcias Corrientes, Chaco, Formosa, Misiones, Santa Fe y Entre Ríos

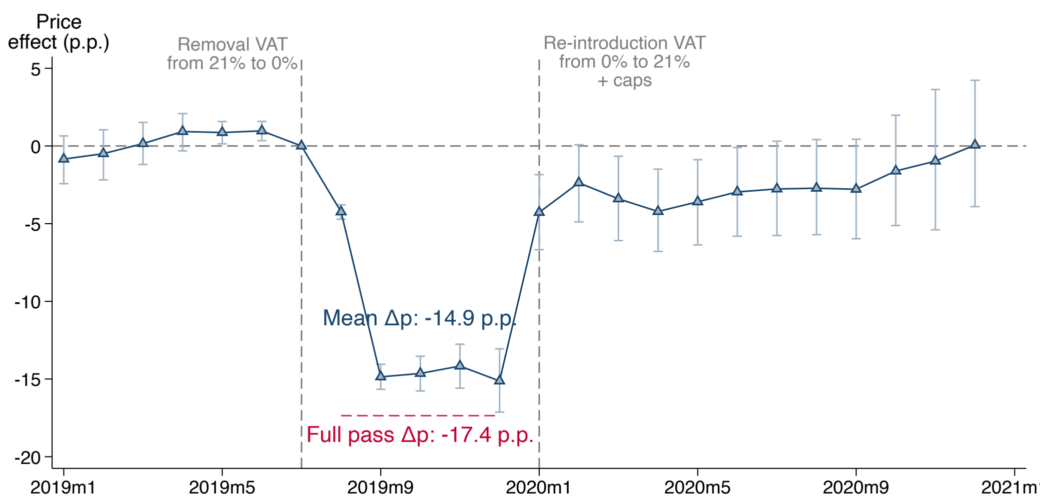
Notes: This figure shows the structure of our geographic variables in our databases. Overall, stores can be located in Gran Buenos Aires (GBA) or the rest of the country (Interior). Within GBA, they can be in the capital of Argentina (Capital Federal) or the rest of GBA area (Periferia). The Interior of the country is classified into: the rest of the province of Buenos Aires (BS AS Resto), Córdoba, Andina region (further split into Cuyo and Northwest NOA), Litoral region (north and south), South, and Austral.

Figure E.7: Price Effect of the Temporary VAT Cut in Chain Supermarkets: Weekly and Monthly Estimates

(a) **Weekly data**



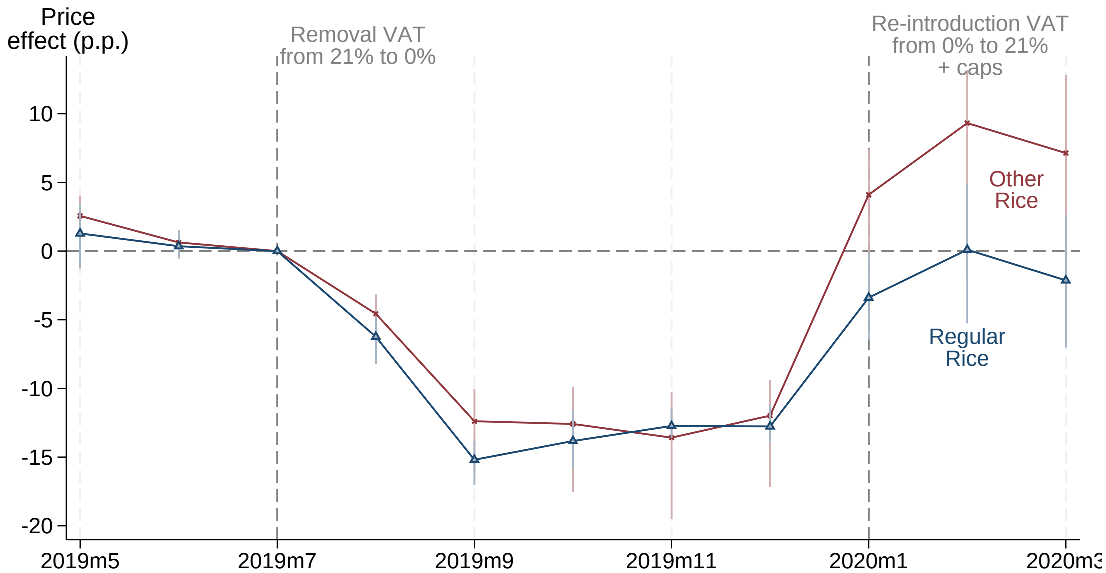
(b) **Monthly data**



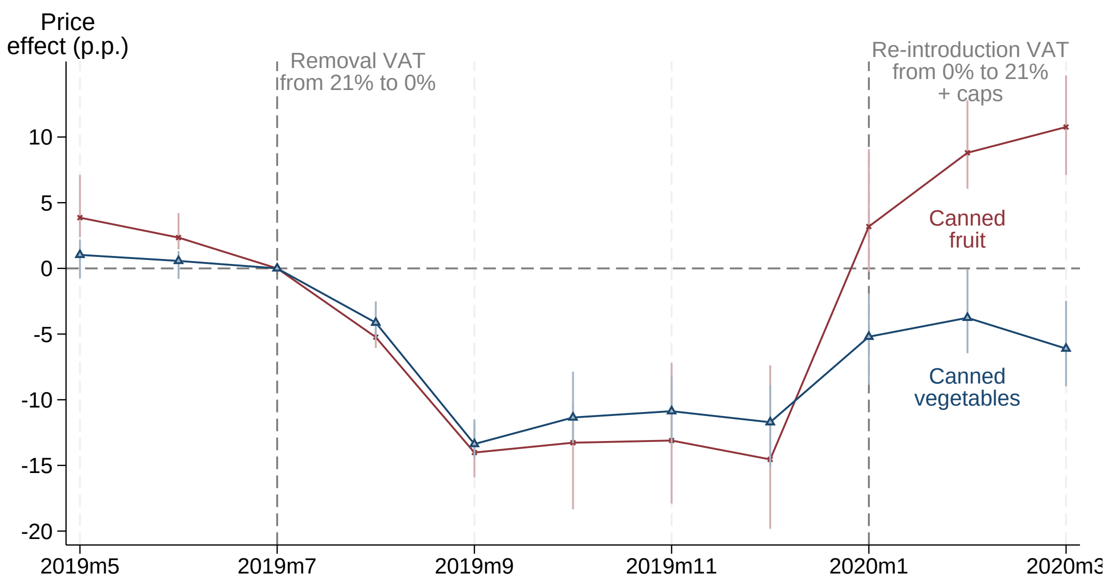
Notes: This figure plots the dynamic difference-in-differences coefficients from equation (1) for all treated goods in chain supermarkets, estimated at the weekly (Panel (a)) and monthly (Panel (b)) frequency. The dependent variable is the price of each barcode normalized to 100 in the week or month before the VAT cut. Aggregating to the monthly level barely affects the estimates or general trends. The first vertical dashed line indicates the VAT cut and the second the VAT increase; the red dashed line marks full pass-through of the cut [$(1 - 1.21)/1.21 \times 100 = -17.4\%$]. Bands are 95% confidence intervals (Panel (b) uses the wild cluster bootstrap; at the weekly frequency in Panel (a) we report the analytic cluster-robust interval).

Figure E.8: Regulated VAT increase with capped pass-through rates

(a) **7% cap (regular rice) versus no cap (other rice)**

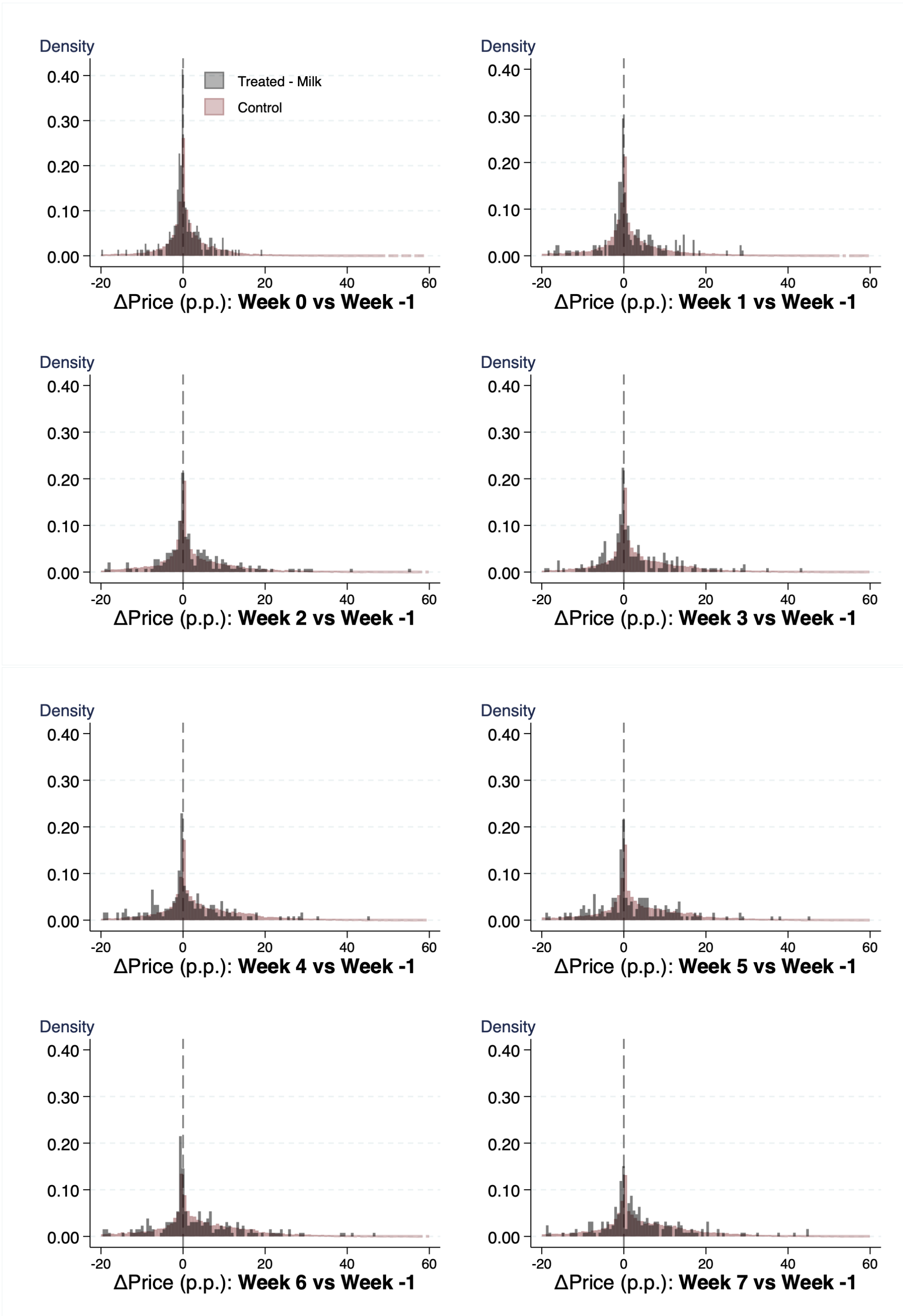


(b) **7% cap (canned vegetables) versus no cap (canned fruit)**



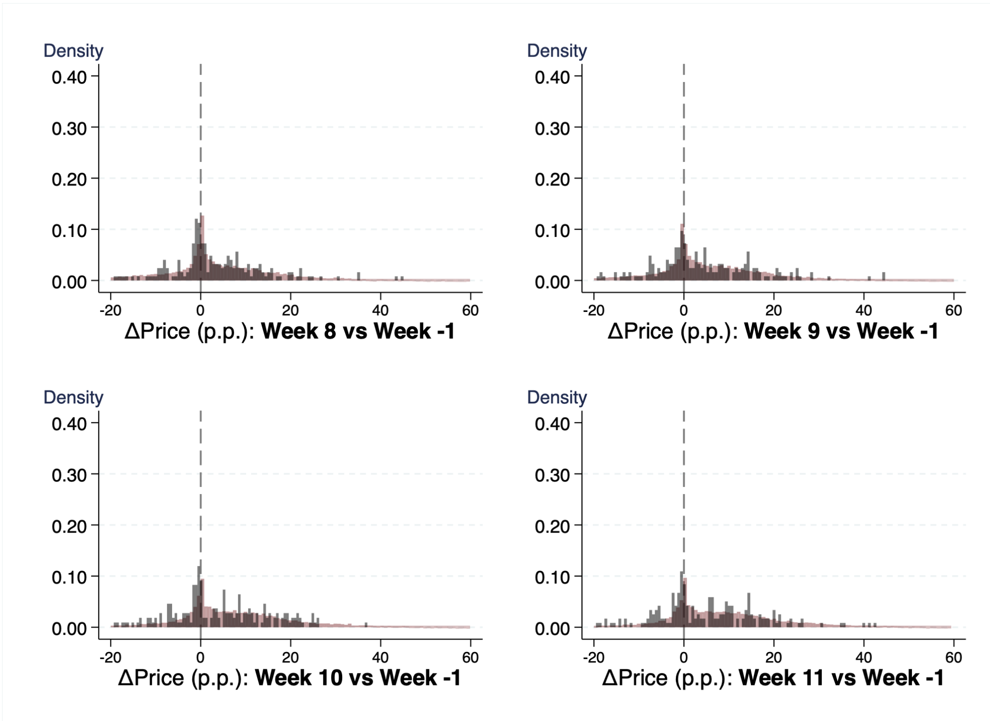
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) in chains before and after the VAT cut and its subsequent repeal. We break down the list of barcodes from the treatment group into food categories that are subject to a capped price increase and food categories with no cap in their price increase (i.e., green light to flexibly increase prices). We compare each group relative to food products in the original control group. For a list of the different caps across categories see Table 2. The dependent variable is the price of each barcode normalized to 100 in the week before the VAT was cut. Panel (a) compares the change in prices for regular rice products subject to the 7% price increase cap and other rice products that are fully flexible (relative to the original control group). Panel (b) compares the change in prices for canned vegetables subject to the 7% price increase cap and canned fruit that are fully flexible (relative to the original control group). The first vertical dashed line indicates the time when the VAT was reduced to 0% for goods in the treatment group. The second vertical dashed line indicates the time when the VAT was reinstated at 21% for goods in the treatment group with differential caps in the allowed price increase.

Figure E.9: Distribution of Price Changes in Chain Supermarkets Following the VAT Increase (milk only)



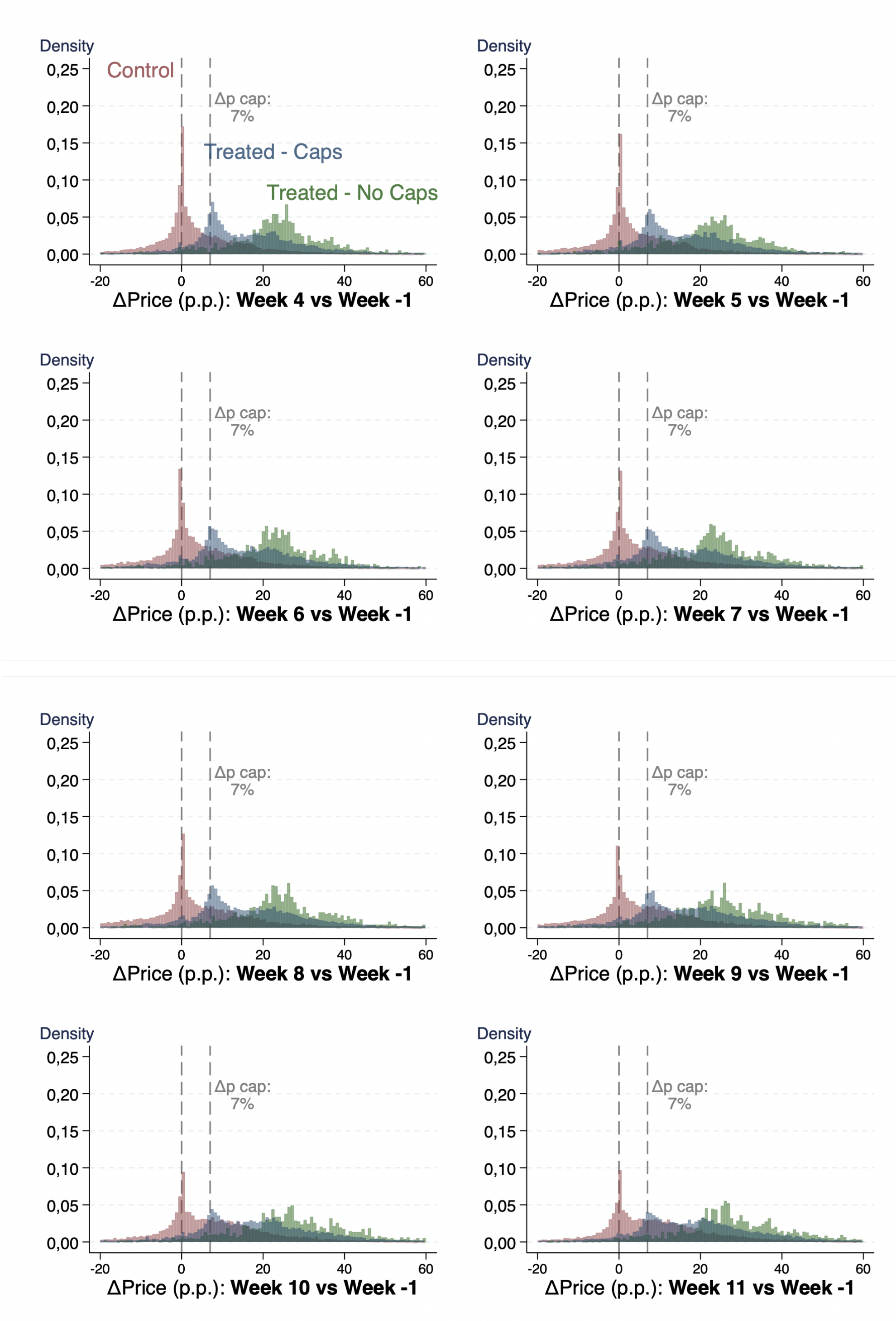
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods in the control group and only milk in the treated one (subject to a 0% price increase cap). The reference week ($week-1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 0 means the first week with a rate of 21%, $Week$ 2 the second, and so on. In each chart, we include a vertical dashed line to illustrate the situation of barcodes with no price changes ($\Delta 0\%$).

Figure E.10: Distribution of Price Changes in Chain Supermarkets Following the VAT Increase (milk only)



Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods in the control group and only milk in the treated one (subject to a 0% price increase cap). The reference week ($week-1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 8 means nine weeks with a rate of 21%, $Week$ 9 ten, and so on. In each chart, we include a vertical dashed line to illustrate the situation of barcodes with no price changes (Δ0%).

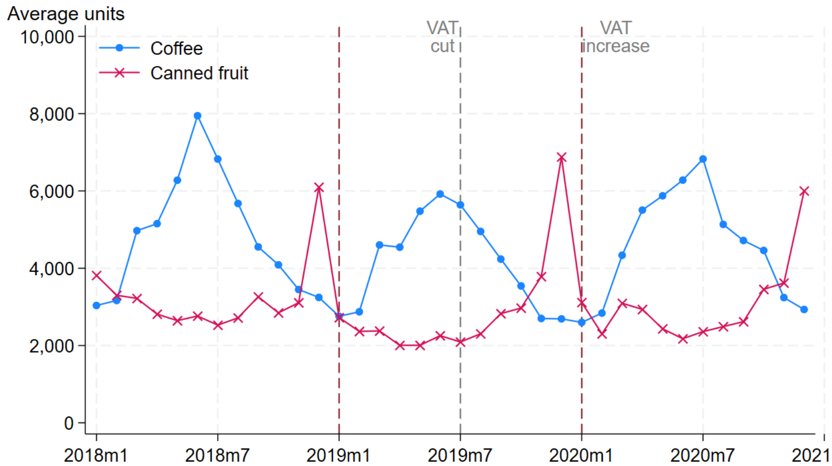
Figure E.11: Distribution of Price Changes in Chain Supermarkets Following the VAT Increase (all goods, excluding milk)



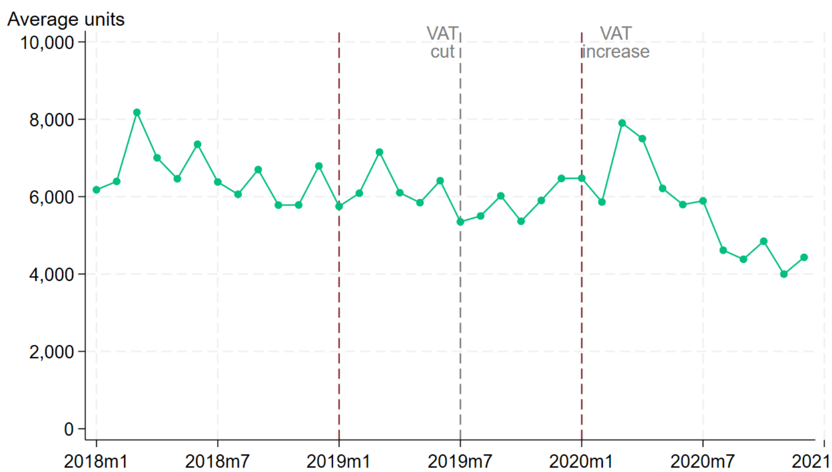
Notes: These figures show the distribution of price changes (at the barcode level) before and after the VAT increase for all goods (excluding milk, which was subject to a 0% price increase cap). The reference week ($week - 1$), refers to the last week of the year 2019 (the last week before the VAT rate of 21% was reintroduced). $Week$ 0 means the first week with a rate of 21%, $Week$ 2 the second, and so on. In each chart, we include two vertical dashed lines to illustrate the situation of barcodes with no price changes (Δ0%) and of those with changes of equal size of the cap (Δ7%).

Figure E.12: Seasonality of Treated Goods

(a) Coffee (control) versus canned fruit (treated)

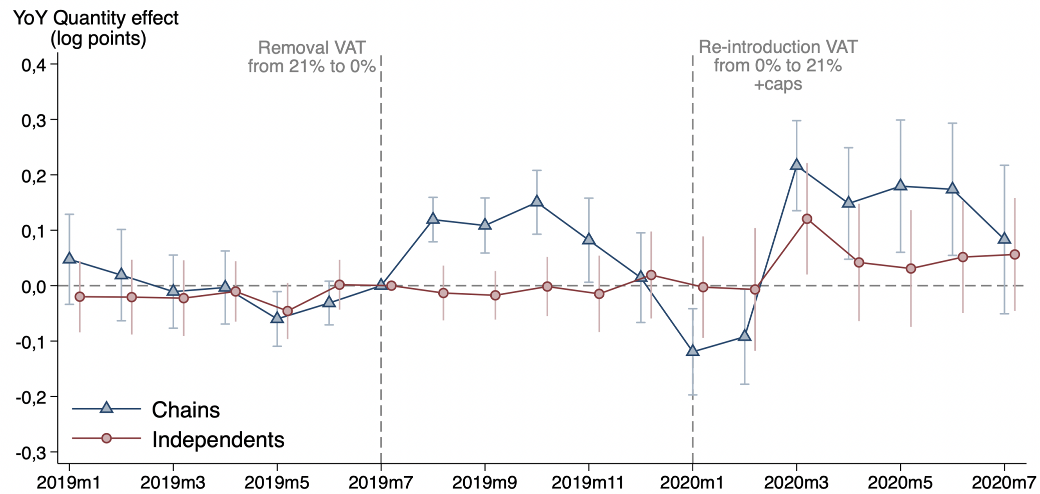


(b) Eggs (treated)



Notes: This figure plots the average monthly quantity sold (in units) over 2018–2020. Panel (a) contrasts coffee, a control good that peaks around mid-year (winter), with canned fruit, a treated good that spikes sharply each December, close to the VAT re-introduction. Panel (b) shows (hen) eggs, another treated good, which by contrast is comparatively flat. The seasonal profile thus varies markedly across treated goods, so a quantity difference-in-differences estimated in levels would conflate seasonality with the treatment effect; this is why we measure quantity responses with a year-over-year transformation (Figure E.13) and why we exclude the most strongly seasonal treated categories—Easter eggs (chocolate) and panettone-style loaf cakes—from the estimation sample. The labeled vertical dashed lines mark the VAT cut (August 2019) and its re-introduction (January 2020).

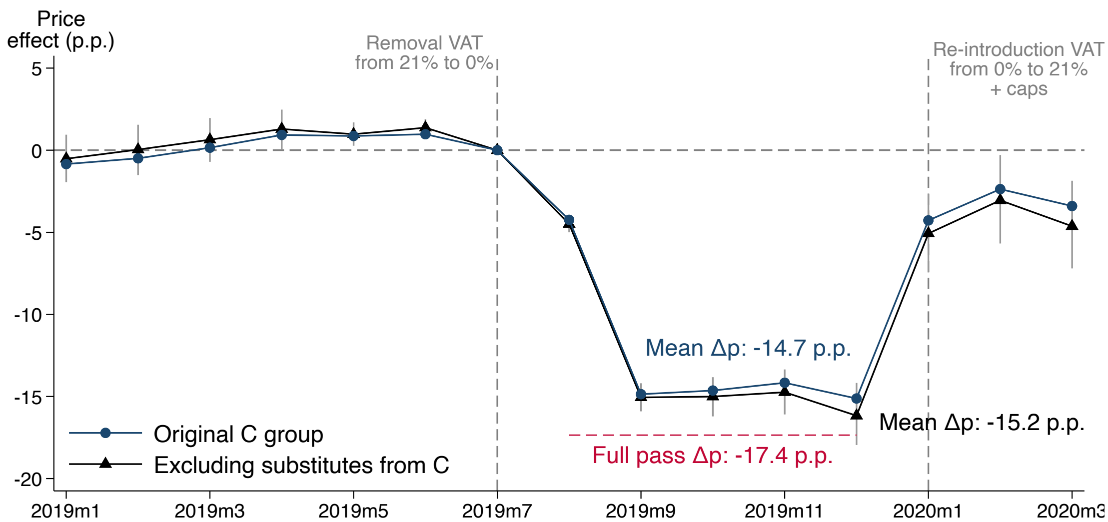
Figure E.13: Quantity Effects in Chain and Independent Supermarkets (Year-over-Year)



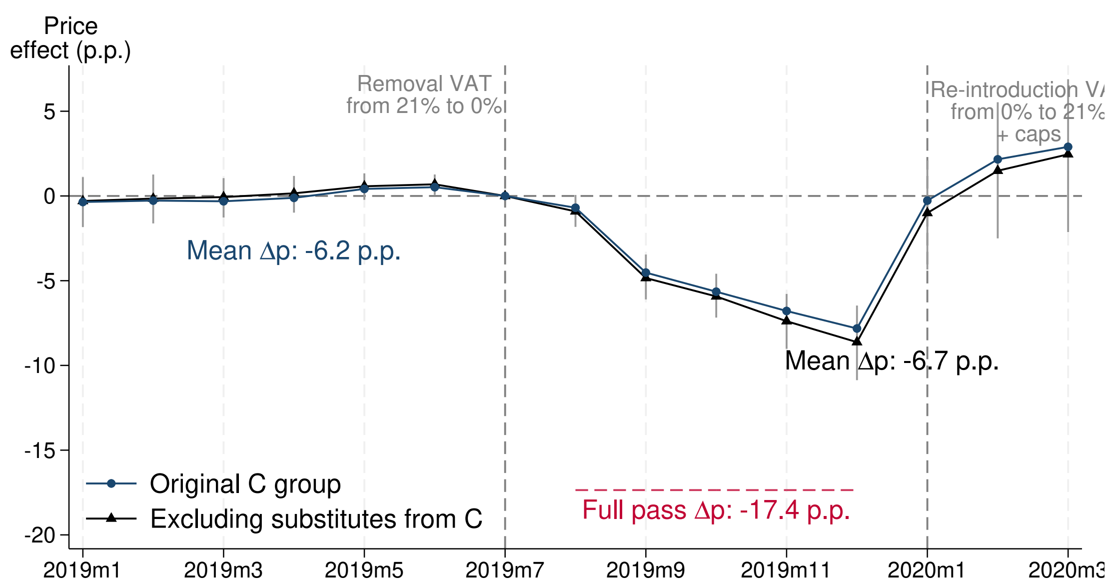
Notes: This figure shows the effect of the VAT cut on the quantity of treated goods sold, for chain supermarkets (navy) and independent stores (maroon). The outcome is the year-over-year change in log unit sales, $\Delta^{12} \log Q_{i,r,s,t} = \log Q_{i,r,s,t} - \log Q_{i,r,s,t-12}$, which differences out any fixed annual seasonal pattern. The specification is the dynamic difference-in-differences in equation (1), with the unit of observation at the barcode $\times$ geographic-area $\times$ store-type $\times$ month level, with barcode-area and month-year fixed effects. The sample consists of food products with at least 20 units sold during January–July 2019, excluding goods affected by the *Precios Esenciales* and *Precios Cuidados* programs; it ends in July 2020 because an August 2020 year-over-year comparison would mechanically involve the August 2019 treatment-onset month. The dashed lines mark the VAT cut (August 2019) and its re-introduction (January 2020); vertical bars are 95% confidence intervals.

Figure E.14: Robustness to Excluding Close Substitutes: Chains and Independent Supermarkets

(a) **Chain supermarkets**

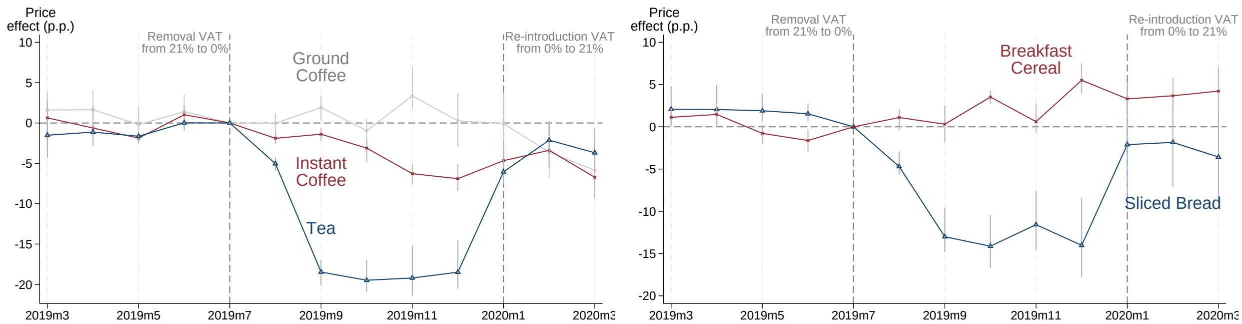


(b) **Independent supermarkets**



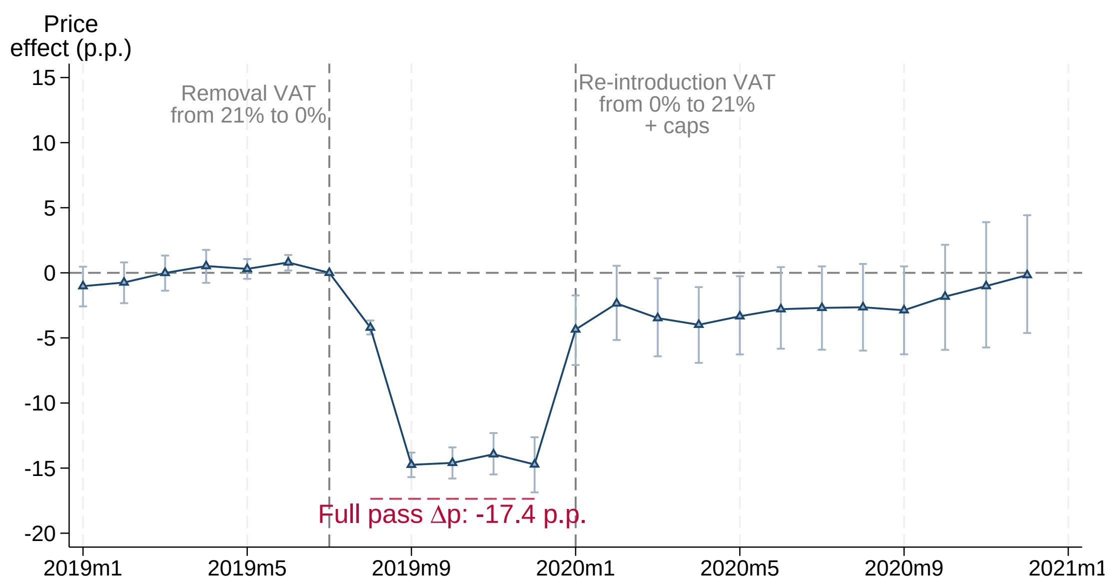
Notes: This figure re-estimates the dynamic difference-in-differences specification (1) on prices using a control group that excludes close substitutes of treated goods (cooking oil, rice, coffee, dried legumes, flour derivatives, soup, and prepared pasta), separately for chain supermarkets (Panel (a)) and independent supermarkets (Panel (b)). In each panel, the navy series is the original control group and the black series excludes close substitutes; both are monthly dynamic difference-in-differences coefficients relative to July 2019, in percentage points, with 95% wild cluster bootstrap confidence intervals (product × region). The red dashed line indicates full pass-through to prices $[(1-1.21)/1.21 \times 100 = -17.4\%]$. Excluding close substitutes barely changes the estimates in either store type, indicating that substitution is not a meaningful source of bias.

Figure E.15: The extent of substitutability in the control group (case studies)



Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1). We focus on specific treated goods (T) and related goods vis-a-vis the remaining categories in the control group. The left panel estimates the price change for barcodes in tea (T), instant coffee (C), and ground coffee (C). The right panel estimates the price change for barcodes in sliced bread (T) and breakfast cereal (C) relative to the rest of the control goods.

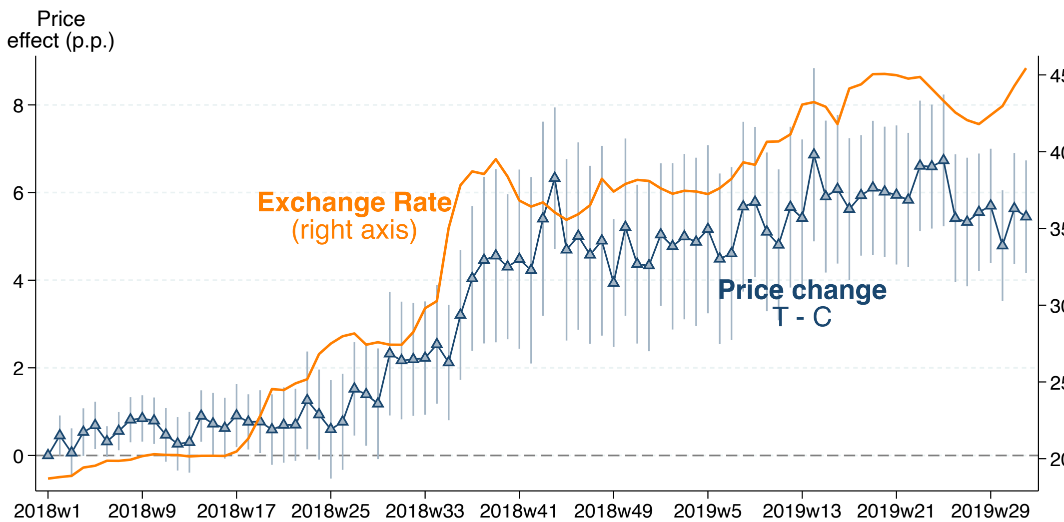
Figure E.16: Excluding imported goods



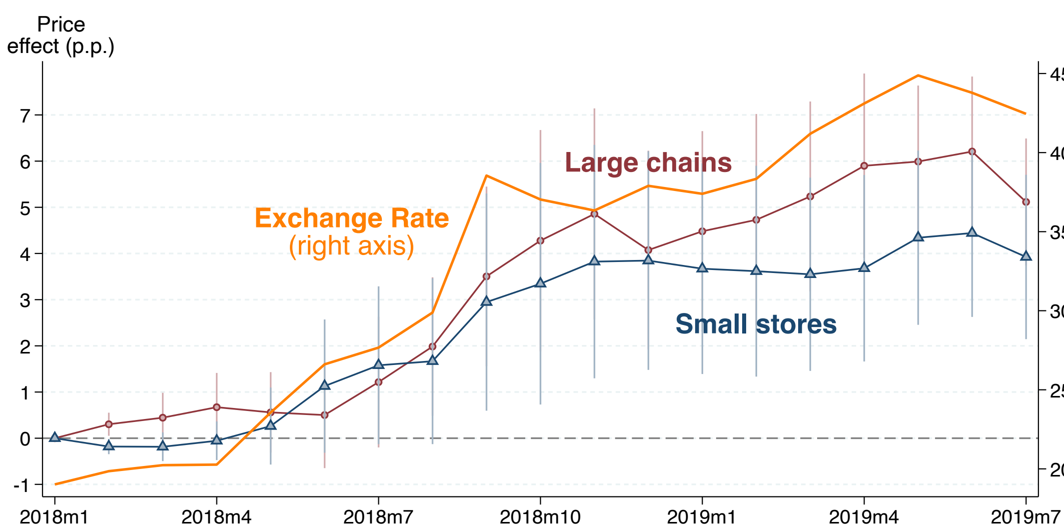
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) before and after the VAT cut and its subsequent repeal in chain supermarkets. In particular, we restrict the estimation sample to those goods that are locally produced and thus are less subject to the large depreciation that happened in mid-August 2019. By construction, barcodes have 13 digits where the first three refer to the GS1 Member Organization of the manufacturer. Although imperfect, this is a good proxy for where the product is manufactured (a firm could be incorporated in a different country than where it manufactures its product). We tag local goods as those with codes 779 and 780, which correspond to Argentina; the remaining codes as imported goods. Considering the full estimation sample, only ten percent are not locally produced, and, interestingly, this percentage is equally split between treated and control goods.

Figure E.17: Pass-through of the 2018 peso depreciation

(a) **Prices of treatment and control in chains**

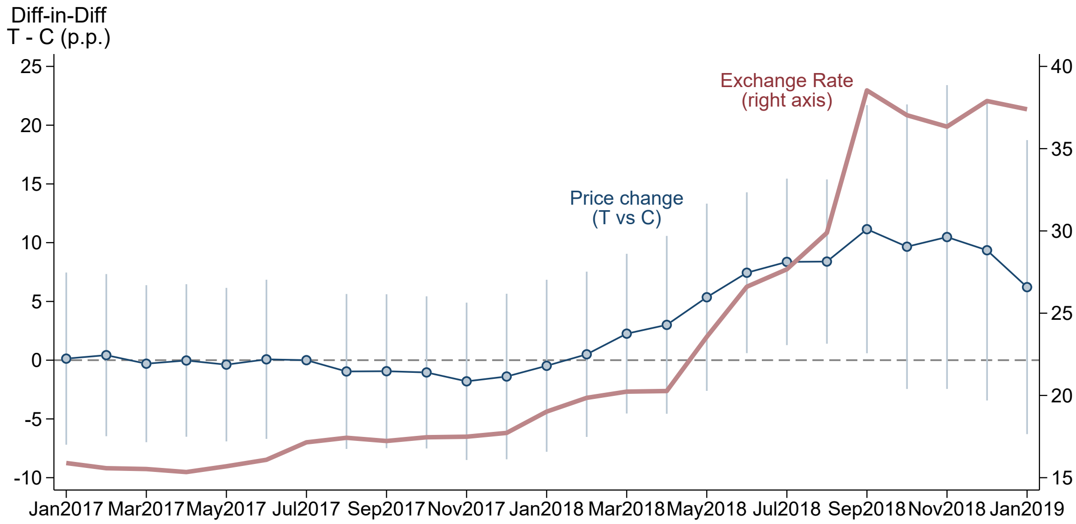


(b) **Prices of treatment and control in independent and chain stores**



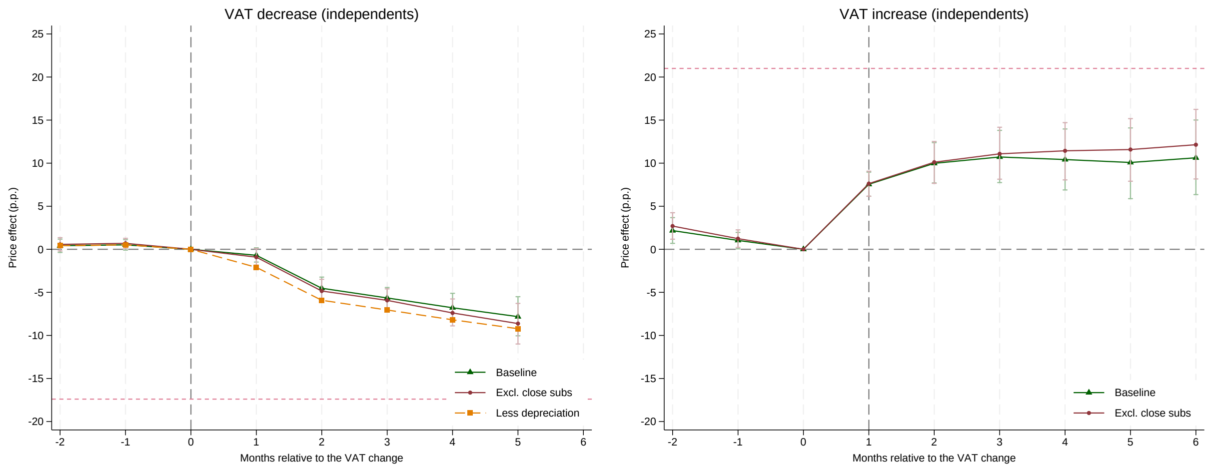
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on the prices of chain and independent supermarkets. The orange line of both charts displays the nominal exchange rate between the Argentine peso and the US dollar (right axis). The blue line in the top panel shows the percentage change in prices relative to week 1 of 2018 between treated and control goods as classified in Table 1. The bottom panel runs the same regression using monthly data in supermarket chains (red line) and independent supermarkets (blue line). All the coefficients are estimated relative to January 2018 (the omitted month in the regression). In Section 4.4, we explain that the effect of the depreciation does not pose a threat to our subsequent findings of the VAT holiday. See Figure E.33 for the price effects spanning the full period 2018-2020.

Figure E.18: Pass-through of the 2018 peso depreciation using aggregate data from INDEC



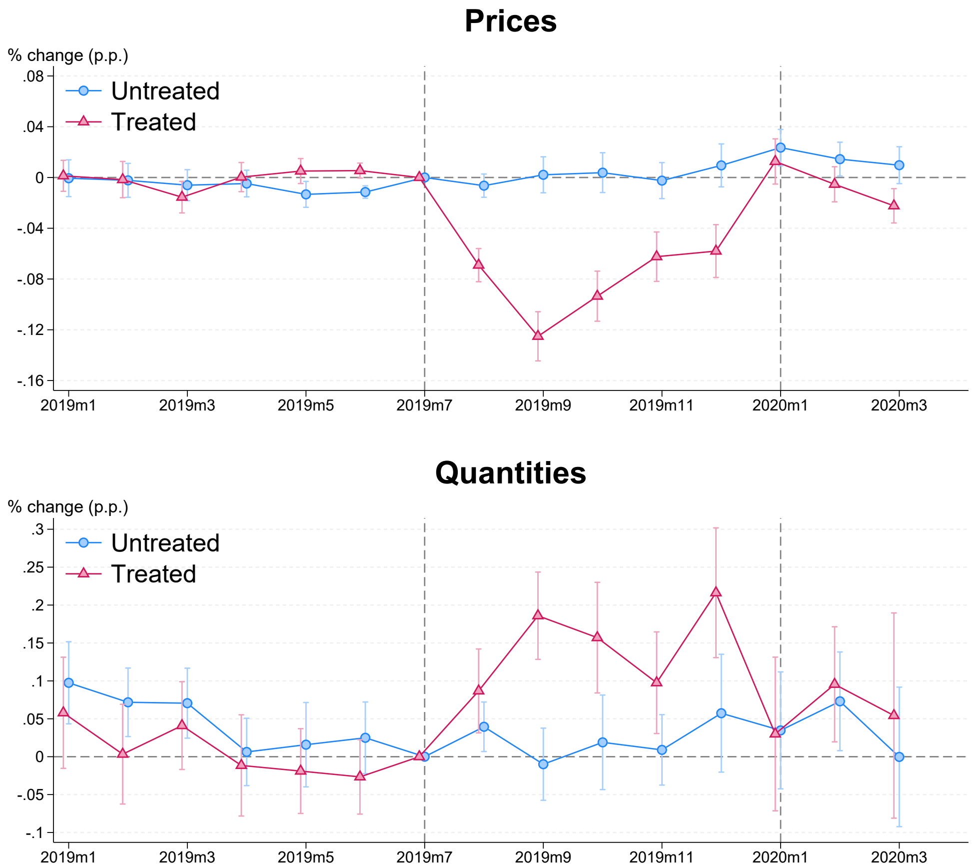
Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) using official aggregate price data from INDEC. The pink line displays the nominal exchange rate between the Argentine peso and the US dollar (right axis). The blue line shows the percentage change in prices relative to week 1 of 2018 between treated and control goods as classified in Table 1 (for the categories available in the basket used to construct the CPI).

Figure E.19: Price Effects in Independent Supermarkets Using Alternative Specifications



Notes: This figure plots the dynamic difference-in-differences price effect in independent supermarkets, estimated as in Figure 2 (same sample and wild cluster bootstrap), on a common event-time axis (months relative to each VAT change). The left panel shows the VAT decrease (normalized to the pre-cut month, July 2019); the right panel shows the VAT increase (normalized to the pre-increase month, December 2019). Each panel reports the baseline specification and a control group that excludes close substitutes of treated goods; the decrease panel additionally reports the baseline netting out the effect of the peso depreciation (a level shift that cancels once the increase is renormalized to its own pre-period). The cranberry dashed lines mark full pass-through (-17.4 p.p. for the cut and $+21$ p.p. for the increase). Across all specifications, pass-through in independent supermarkets remains well below full pass-through for both the VAT cut and its re-introduction. The non-food control specification we report for chains in Figure 5 cannot be replicated for independent supermarkets, because Scentia's main data extract does not include non-food items for independents.

Figure E.20: Income Effects



Notes: These figures plot the effect of the VAT changes on the difference between chain and independent supermarkets. We do the analysis separately for log(prices) and log(quantities), top and bottom panels, respectively. Also, we run separate regressions, one where we focus on goods that are not directly treated by the VAT cut (untreated, including non-food items) and another on goods directly treated by the VAT cut (treated) in chain versus independent supermarkets.

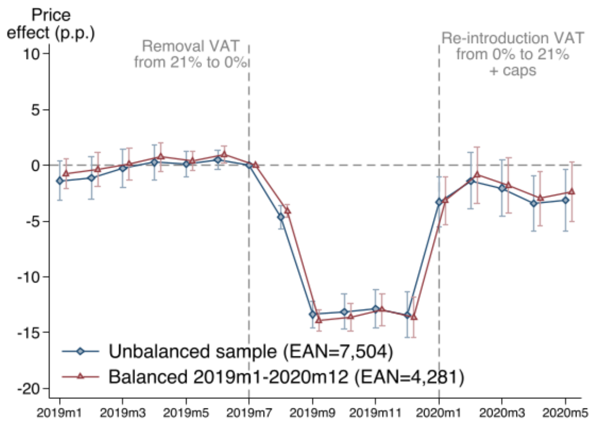
E.1 Sample Restriction and the Extensive Margin

Our main estimation sample restricts to a balanced panel of barcodes with positive sales throughout the estimation window. This appendix documents that the restriction does not drive our pass-through estimates and that the reform did not differentially affect the entry or exit of treated relative to control goods. We prefer the balanced panel because scanner data contain a large mass of low-frequency products that are sold only sporadically. Such cells contribute disproportionately to mechanical entry and exit while carrying negligible sales weight, so including them in an unbalanced panel introduces noise from sporadic product turnover with little economically meaningful variation.

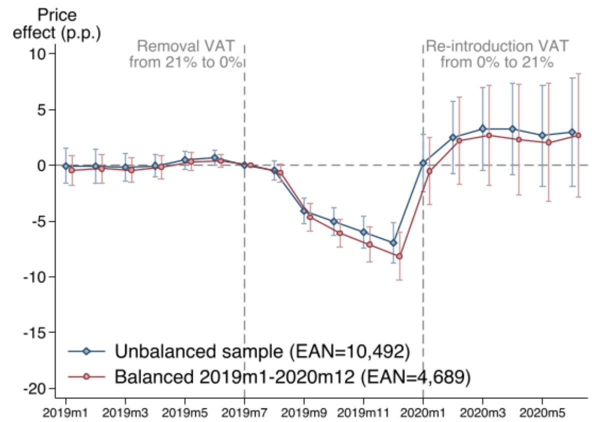
Appendix Figure E.21 re-estimates equation (1) on the unbalanced sample, which retains all barcodes rather than only those continuously present, and overlays it on our balanced-panel estimate, separately for chain and independent supermarkets. The two series are nearly indistinguishable in both store types, so the balanced-panel restriction does not materially affect the estimated pass-through. Appendix Figure E.23 plots Kaplan–Meier survival curves for treated and control products from the July 2019 risk set; the two groups leave the data at very similar rates, with no divergence around either VAT change. Finally, Appendix Figure E.22 examines the extensive margin directly, reporting a dynamic difference-in-differences on an indicator for whether a barcode-region-store-type cell records strictly positive sales in a given month, for treated relative to control goods. The coefficients are flat and centered on zero throughout, with no post-reform decline, indicating that treated goods do not differentially disappear from the data after the reform.

Figure E.21: Price Effects with Unbalanced and Balanced Samples

(a) **Chain Stores**

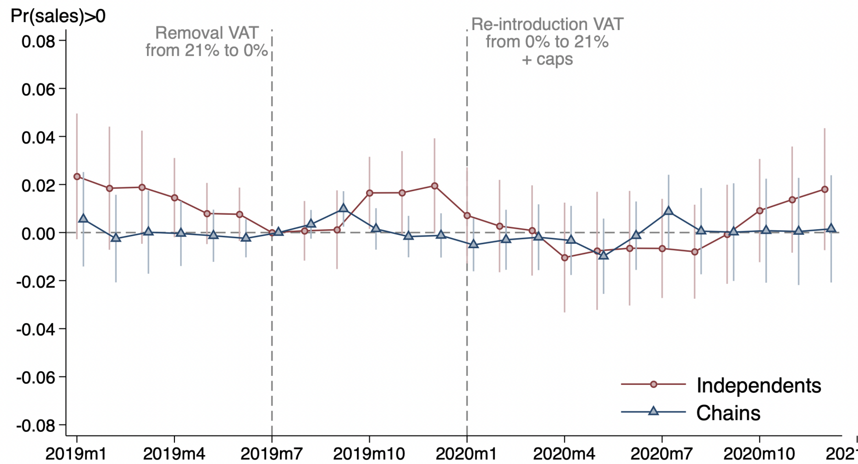


(b) **Independent Stores**



Notes: This figure plots the dynamic difference-in-differences coefficients from equation (1) using an unbalanced sample of barcodes (black) and a balanced sample of barcodes with positive sales in every month from January 2019 through December 2020 (blue for chains, red for independents). Barcodes are grouped into treatment and control as in Table 1, and prices are normalized to 100 in the month before the VAT cut. The first vertical dashed line marks the reduction of the VAT to 0% and the second its reinstatement at 21%. Bands are 95% wild cluster bootstrap confidence intervals. Easter eggs and panettone-style loaf cakes (both highly seasonal) and toasts (recorded only in chain supermarkets) are excluded.

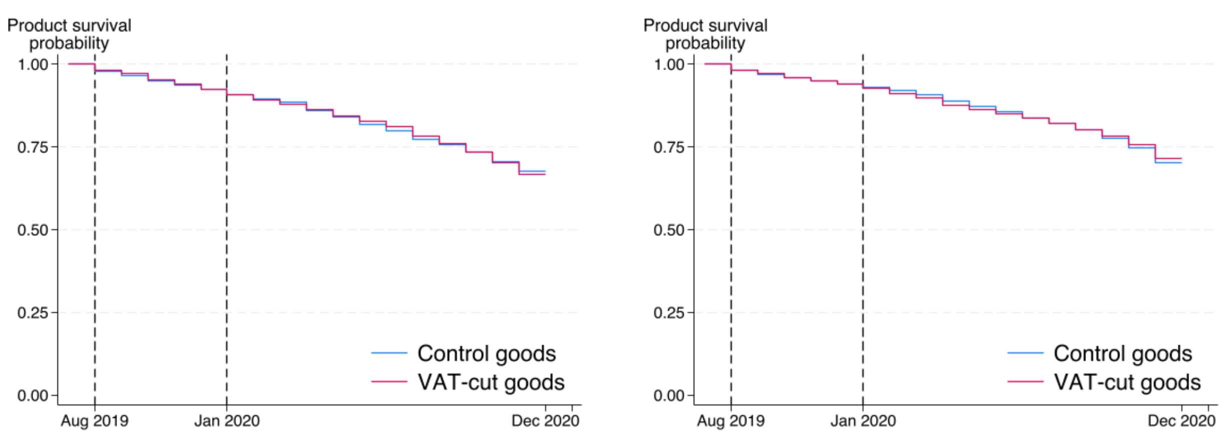
Figure E.22: Extensive-Margin Product Retention



Notes: This figure plots dynamic difference-in-differences estimates of the differential probability that a product-area-store-type cell is active in a given month for VAT-cut goods relative to control goods, estimated separately for chain (navy) and independent (maroon) supermarkets. The sample starts from the set of cells observed before the VAT cut; a cell is active if it records strictly positive sales. Coefficients are normalized to zero in July 2019. Easter eggs and panettone-style loaf cakes (both highly seasonal) and toasts (recorded only in chain supermarkets) are excluded. Flat coefficients centered on zero indicate no differential post-reform exit of treated goods.

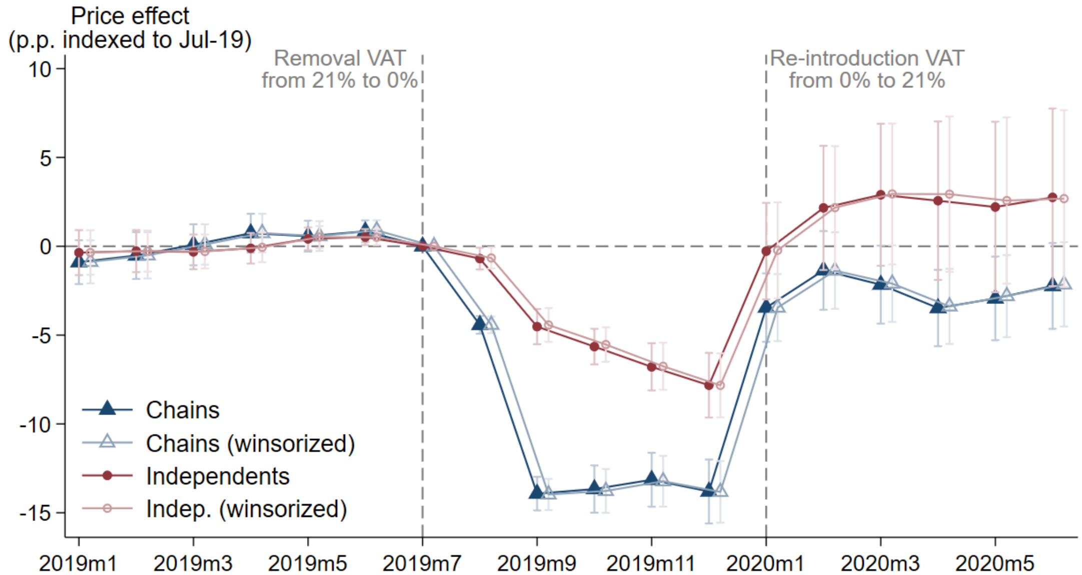
Figure E.23: Kaplan–Meier Product Survival Around the VAT Changes

(a) **Chain Stores** (b) **Independent Stores**



Notes: Kaplan–Meier survival curves for barcode–area cells observed in July 2019, separately for treated and control goods. The July 2019 risk set comprises 18,449 control and 11,570 VAT-cut cells in chain stores (5,035 and 3,513 distinct barcodes) and 16,748 control and 11,555 VAT-cut cells in independent stores (6,961 and 5,005 distinct barcodes). Failure is defined as permanent disappearance from the data, dated to the first month after a product is last observed; products still present in December 2020 are right-censored. The vertical lines mark the VAT cut (August 2019) and the return to 21% (January 2020). Easter eggs and panettone-style loaf cakes (both highly seasonal) and toasts (recorded only in chain supermarkets) are excluded.

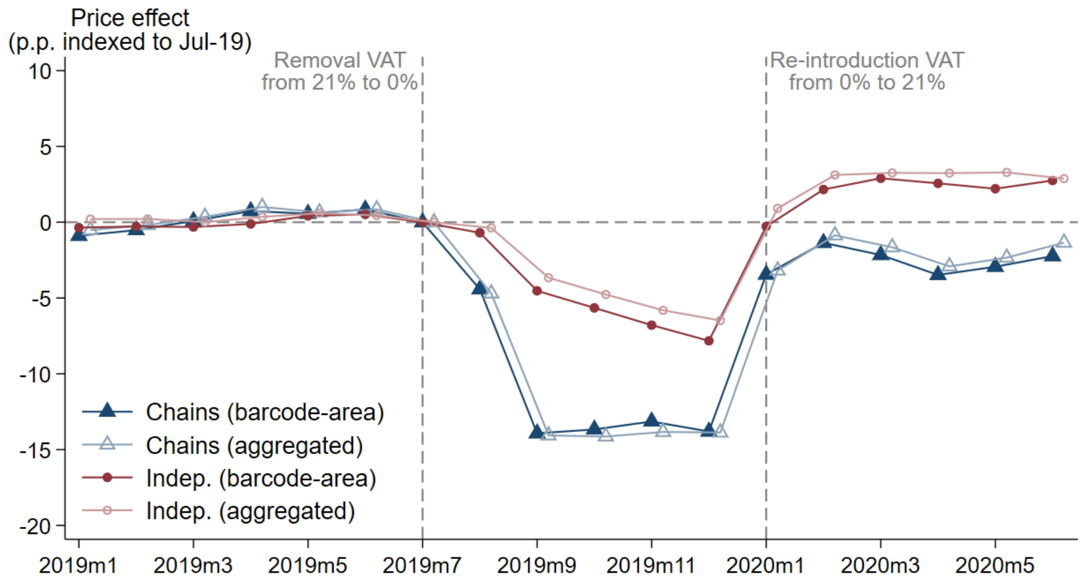
Figure E.24: Robustness: Pass-Through Estimates with Winsorized Price Changes



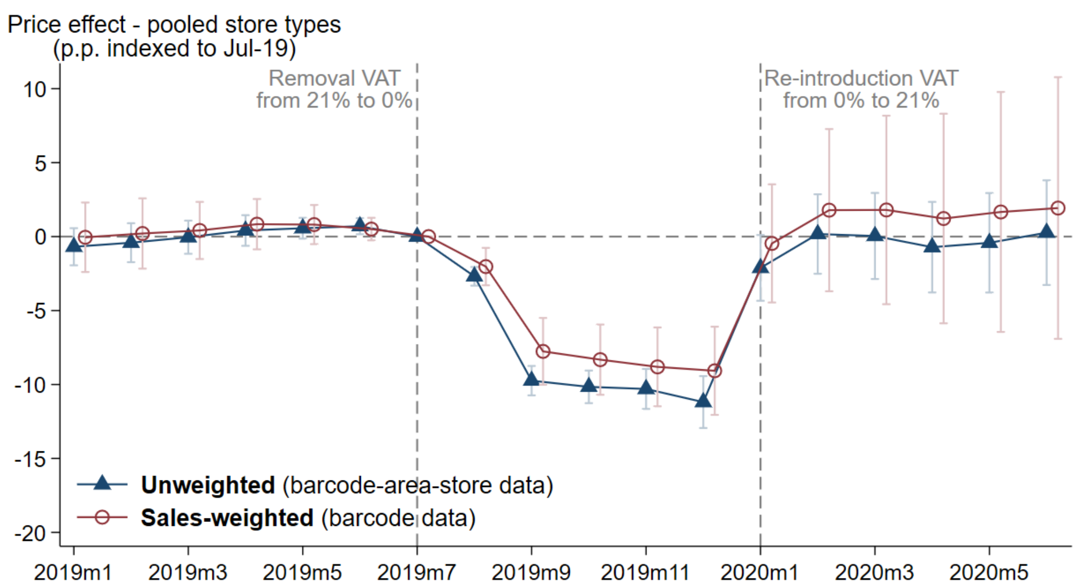
Notes: This figure overlays the dynamic difference-in-differences pass-through estimates from equation (1) using the baseline (unwinsorized) price outcome (solid markers) and the same specification with the price outcome winsorized at the 1st and 99th percentiles within each month × treatment-status × store-type cell (hollow markers), separately for chain (navy) and independent (maroon) supermarkets. Prices are indexed to 100 in July 2019; vertical bars are 95% confidence intervals. The near-complete overlap between the winsorized and unwinsorized estimates—including in the pre-reform months—indicates that the pass-through magnitudes are not driven by extreme observations in the tails of the price-change distribution.

Figure E.25: Robustness: Equal-Weighted vs. Sales-Weighted Aggregation

(a) **By store type**

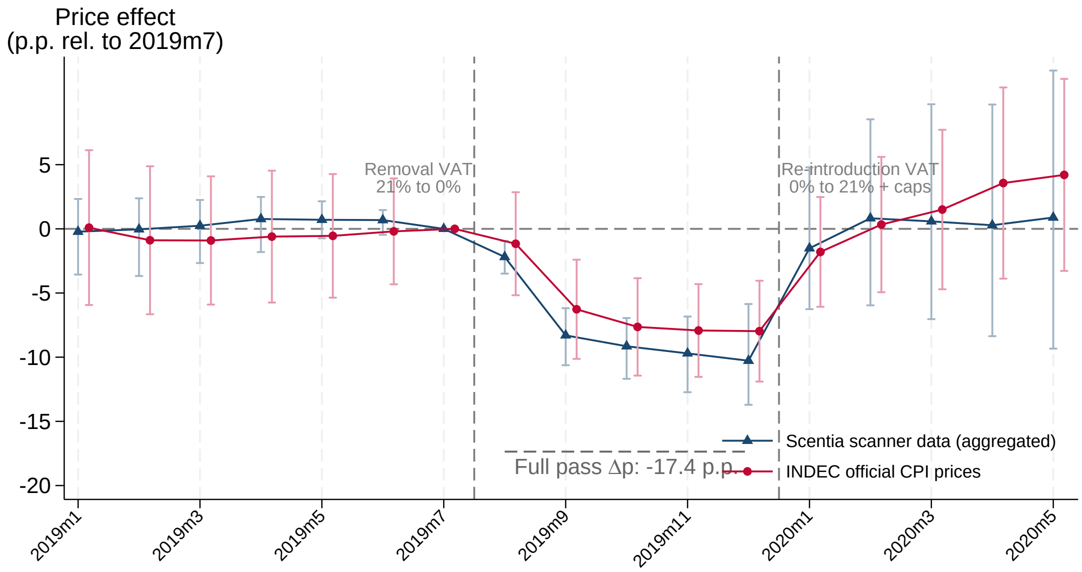


(b) **Pooled across store types**



Notes: Both panels show that weighting the data by sales has little effect on the estimated pass-through. In Panel (a), estimated separately by store type, the filled series weight each barcode–region–store-type–time cell equally (our baseline) and the open series replace this with sales weights: for each barcode and store type, we sum units and revenue across all geographic areas, recompute the unit value as total sales divided by total units, re-index to July 2019 = 100, and re-estimate, so that each area contributes in proportion to its sales volume. Navy denotes chain supermarkets and maroon denotes independent supermarkets. Panel (a) reports point estimates only, because the sales-weighted aggregation collapses the regional dimension and the product–region clustering we use for inference elsewhere cannot be applied to it. Panel (b) pools across both store types: the navy series is the equal-weighted baseline (all barcode–region–store-type cells with cell-level fixed effects) and the maroon series collapses the data to a single sales-weighted national price index per barcode (summing units and revenue across all geographic areas and both store types, recomputing the unit value, and re-indexing to July 2019 = 100); both are dynamic difference-in-differences coefficients relative to July 2019, in percentage points, with 95% confidence interval bands. In both panels the equal-weighted and sales-weighted series track each other closely, indicating that the estimated pass-through is not driven by a few high- or low-volume local markets or by the choice of weighting scheme.

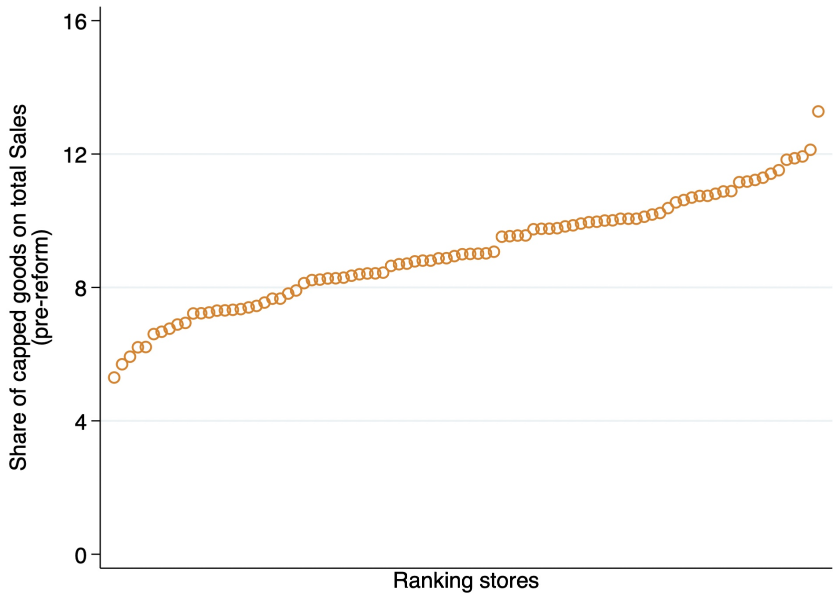
Figure E.26: Robustness: Pass-Through Using INDEC Aggregate Price Series



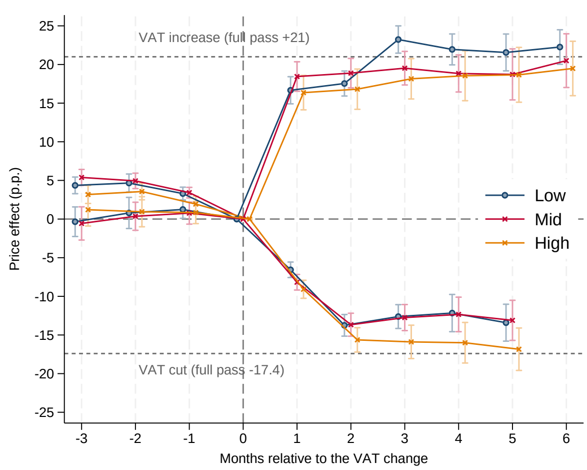
Notes: This figure overlays our scanner-data difference-in-differences estimate (navy) with the same specification estimated on aggregate price series produced by the National Institute of Statistics and Censuses (INDEC, cranberry). To make the two series comparable in their level of aggregation, the scanner series here is aggregated to the national-by-store-type level (regions collapsed, with unit values recomputed from total quantities and sales), matching INDEC's national aggregation. The INDEC series is estimated on INDEC's monthly CPI categories, classifying each category as treated or control; INDEC constructs these series with the representative expenditure weights it uses for the Consumer Price Index, so the INDEC estimate captures a representatively weighted average comparable to our aggregated scanner estimate (Figure 1c). Both series are dynamic difference-in-differences coefficients measured relative to July 2019, in percentage points; bands are 95% confidence intervals (wild cluster bootstrap at the product level—category × treatment, the analog of product–region once regions are collapsed—for the scanner series, and heteroskedasticity-robust for the INDEC series), and the INDEC markers are offset slightly to the right for legibility. The dashed horizontal line marks full pass-through of the VAT cut $[(1 - 1.21)/1.21 \times 100 = -17.4\%]$. The two series track each other closely throughout: parallel pre-trends, a sharp drop at the cut that falls well short of full pass-through, and a return toward baseline at the re-introduction.

Figure E.27: Cross-Store Evidence on the Uncapped-Goods Counterfactual: Pre-Reform Exposure to the Price Caps

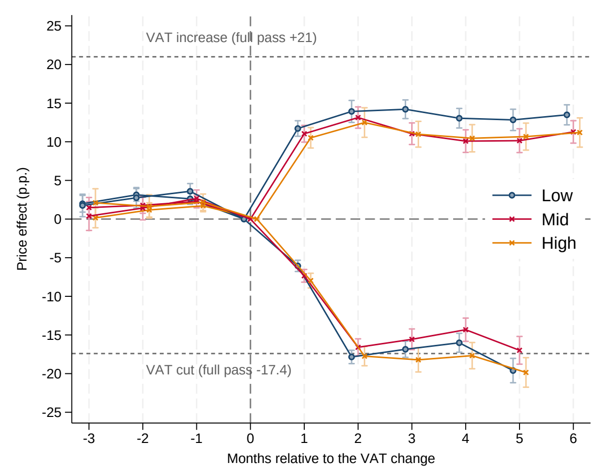
(a) **Pre-reform capped-goods share across stores**



(b) **Pass-through, uncapped treated goods**

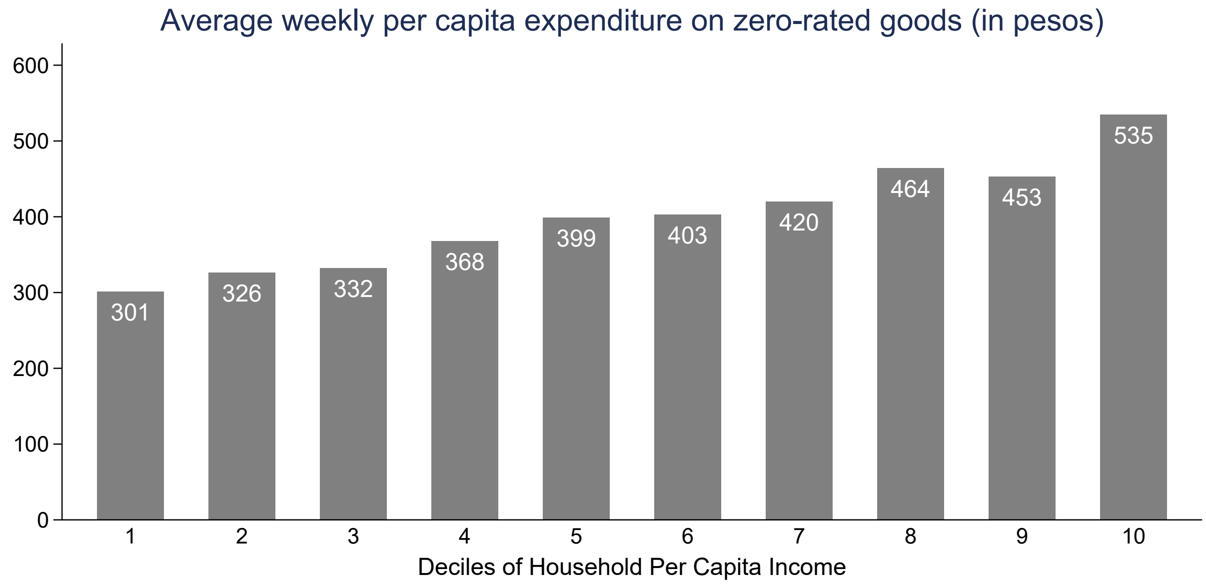


(c) **Pass-through, capped treated goods**



Notes: This figure presents the cross-store intensity test using the Córdoba store-level sample (ninety chain stores, monthly frequency). Stores are ranked by the share of their pre-reform (January–July 2019) sales accounted for by goods later subject to the 7% anti-profiteering cap, and split into Low, Middle, and High intensity terciles (thirty stores each). Panel (a) plots that pre-reform capped-goods share across the ranked stores. Panels (b) and (c) report the dynamic difference-in-differences specification (1) estimated separately within each tercile, with barcode, store, and month fixed effects and standard errors clustered at the subcategory × treatment-group × store level. Both plot the two reforms on a common *event-time* axis, each normalized to its own pre-period—the VAT cut to July 2019 (the downward branch) and the VAT increase to December 2019 (the upward branch); the dashed horizontal lines mark full pass-through of the cut (-17.4 p.p.) and of the increase ($+21$ p.p.). Panel (b) restricts the estimation sample to uncapped treated goods (and the control group); Panel (c) restricts it to capped treated goods. In all panels, Low = navy, Middle = cranberry, High = orange.

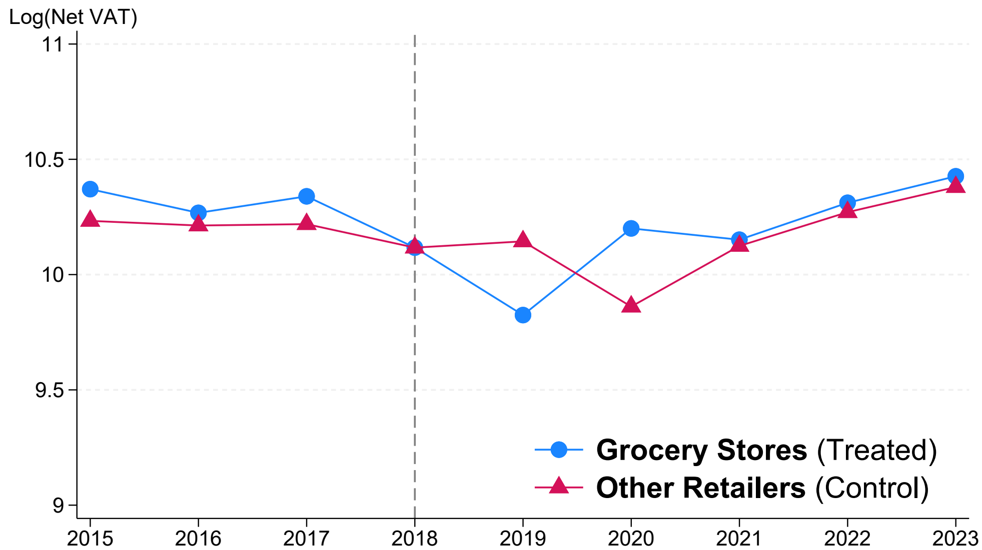
Figure E.28: Per Capita Weekly Expenditure on Zero-Rated Goods, by Income Decile



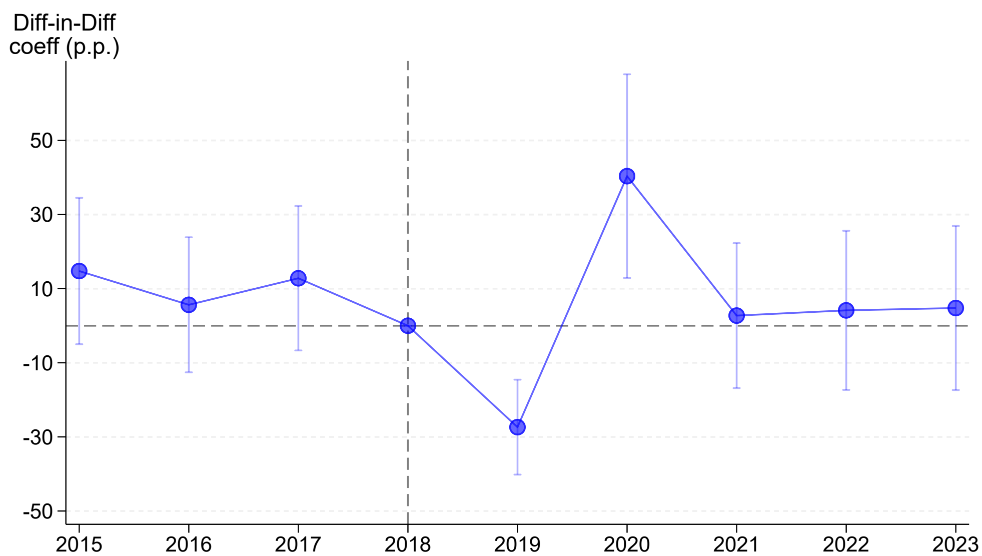
Notes: This figure shows the average per capita household expenditure on zero-rated goods, in Argentine pesos, for the reference week of the survey. Although the share of total expenditure on zero-rated goods declines with income (Figure 6, Panel (a)), the level of peso expenditure on these goods rises with income.
Source: authors' calculations using the 2017/2018 National Household Expenditure Survey (ENGHo).

Figure E.29: Effect on net VAT for affected and non-affected retailers

(a) Levels



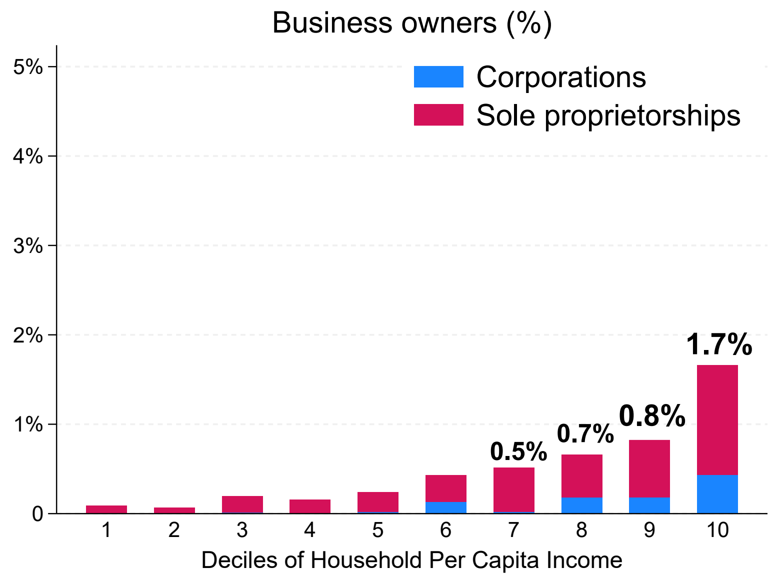
(b) Differences



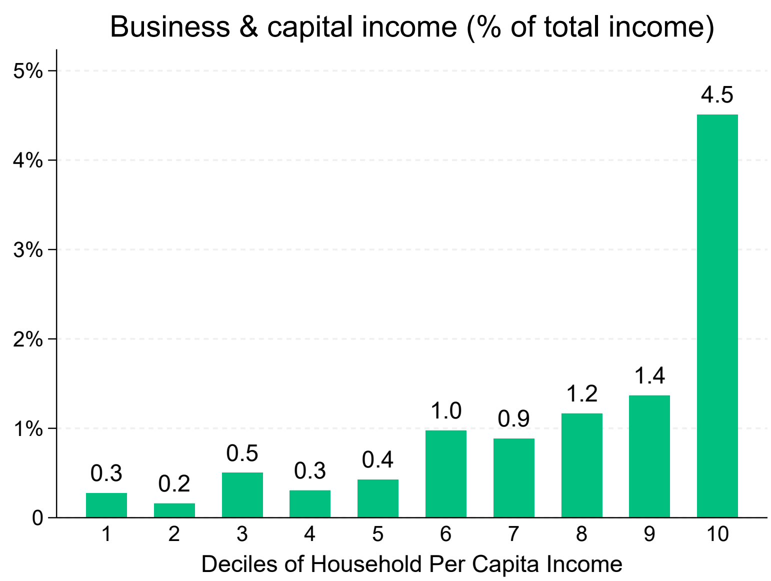
Notes: This figure shows the effect of the temporary VAT cut on VAT liability, as reported in VAT return data. The main outcome variable, Y_{it}, is the log of net VAT—defined as VAT debit from sales minus VAT credit from purchases. The top panel plots its evolution separately for grocery stores (blue circles, treated group) and other retailers (red triangles, control group), as defined in Table A5. All Values are expressed in constant 2019 pesos to account for inflation. To facilitate visual comparison, the series for the treated group has been re-scaled to match the level of the control group in 2019. The bottom panel presents estimated β coefficients from the following difference-in-differences specification: $Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq 2018} \beta_t D_{it} + \epsilon_{it}$, where i indexes retail subsectors, t denotes years, and D_{it} is the treatment indicator. The semi-log coefficients are converted into percentage changes, and robust standard errors are computed using the Delta Method. Regressions are weighted by the number of VAT returns in each sector-year cell. *Source:* Authors' calculations based on ARCA's Statistical Yearbook (FY2015–2023), which reports disaggregated VAT return data for 230 retail subsectors.

Figure E.30: Business and Capital Ownership and Income by Income Deciles

(a) Retail and Wholesale Ownership

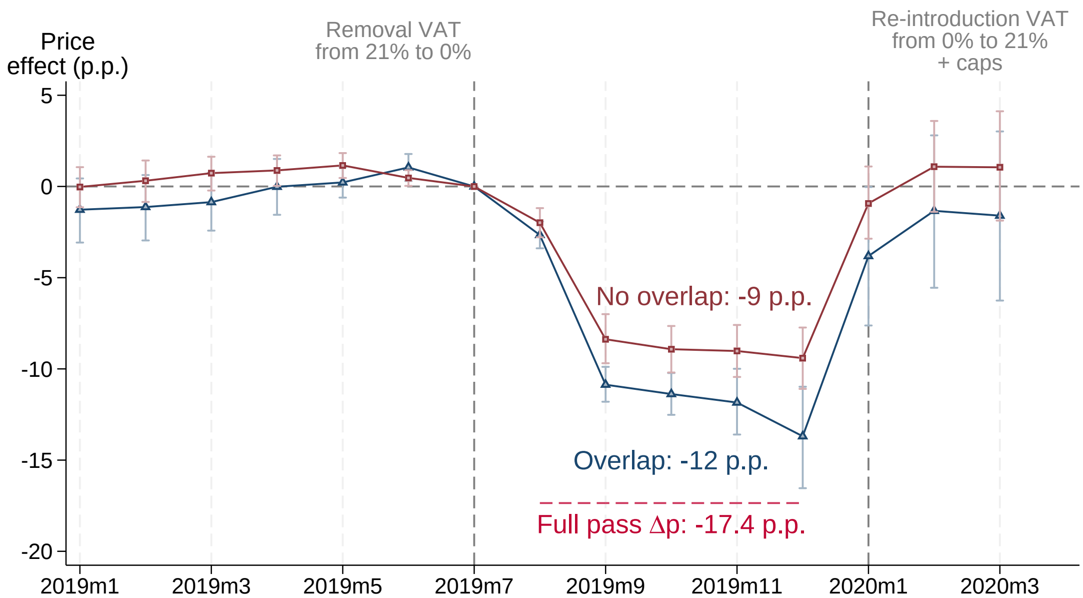


(b) Income Derived from Capital and Business Ownership



Notes: These figures plot the distribution of firm ownership in the "Retail and Wholesale" sector (upper panel) and the distribution of income from businesses and capital sources (lower panel), by income decile. In Panel (a), the ownership indicator captures individuals who report owning a business and employing workers in sector G (Retail and Wholesale). Blue bars represent corporations (e.g., supermarket chains), while red bars represent sole proprietorships (e.g., independent grocery stores). This definition is conservative, as it includes all retail businesses—not only packaged food-related ones like grocery stores. In Panel (b), we compute total income derived from business ownership (e.g., distributed profits, honoraria, salaries) and capital income for the same individuals. This is also a conservative measure, as capital income may include returns from investments in sectors beyond grocery stores—and thus unaffected by the VAT change.
Source: Authors' calculations using the 2017/2018 National Household Expenditure Survey (ENGHo).

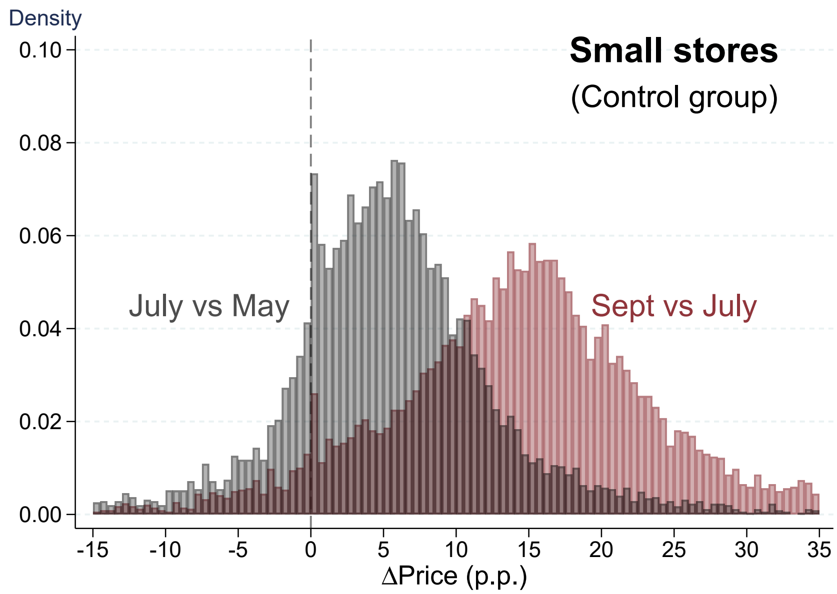
Figure E.31: Price Levels for Barcodes Sold in Both Chain and Independent Supermarkets (overlap) Compared to Barcodes sold in One or the Other (no overlap)



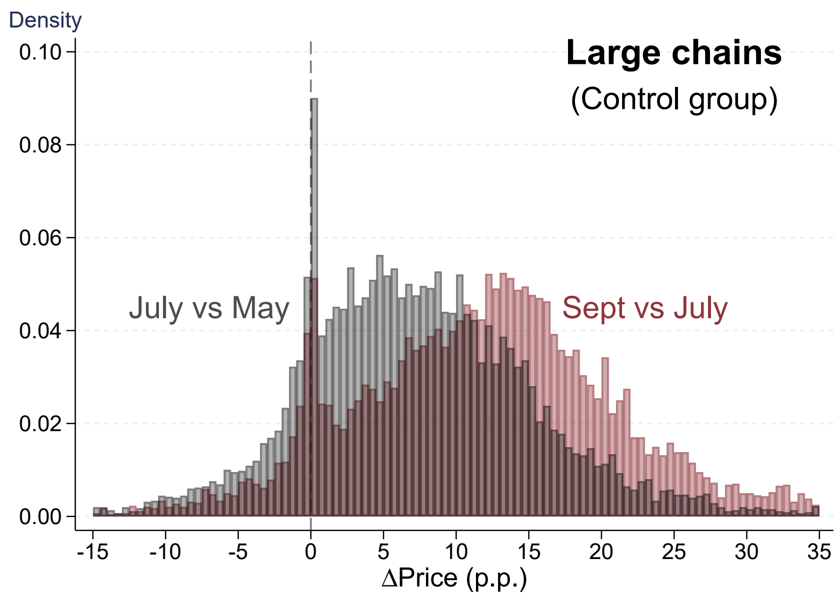
Notes: This figure pools chain and independent supermarkets and breaks down pass-through rate estimates for goods at the barcode level that are sold in both types of supermarkets versus goods that are sold in either one of them but not both. Presumably, goods that are sold in both supermarket and independent chains will be more competitive, leading to higher pass-through rates of the VAT cut.

Figure E.32: Distribution of price changes in independent and chain supermarkets (Depreciation Episode)

(a) Independent stores

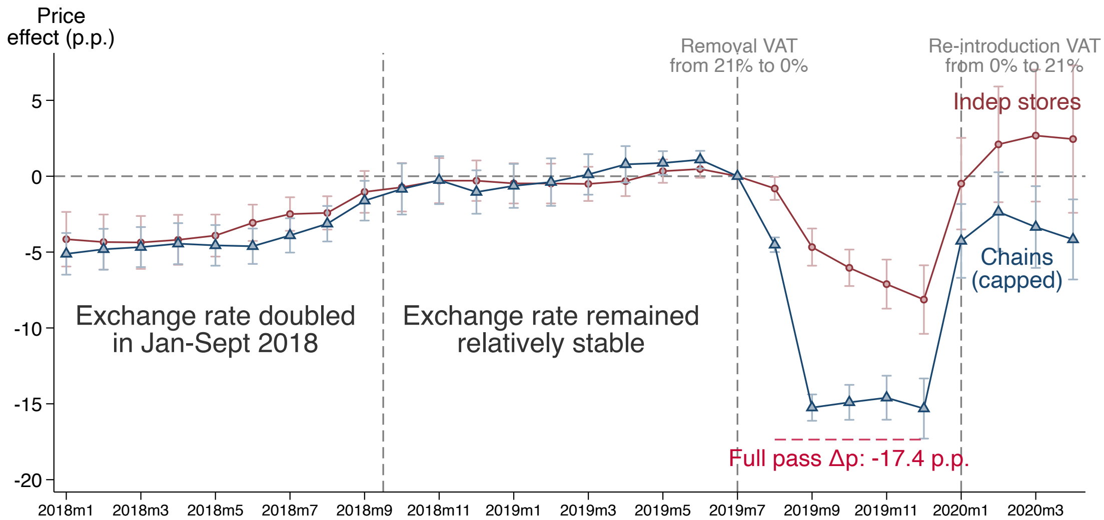


(b) Chain stores



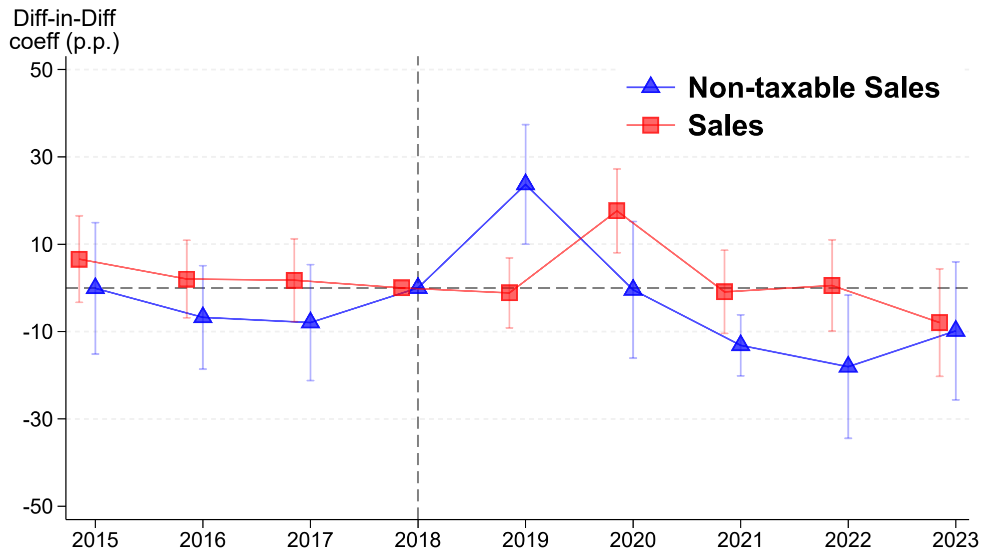
Notes: This figure shows the distribution of price changes in independent supermarkets (top panel) and supermarket chains (bottom panel) for barcodes in the control group. The gray area displays the difference in prices between July and May 2019 *before* the peso depreciation. The red area displays the difference in prices between September and July 2019 *after* the peso depreciation. The figure shows that the prices of goods unaffected by the VAT cut respond similarly in chain and independent supermarkets to other types of macro shocks (the depreciation, in this case).

Figure E.33: Pass-through of the 2018 peso depreciation using scanner data



Notes: This figure shows the results of estimating the dynamic difference-in-differences specification (1) on the prices of chain supermarkets (blue line) and independent stores (red line). We use monthly data for a balanced sample of barcodes with positive sales in the 27 months between January 2018 and March 2020. The effects are estimated relative to July 2019 (the omitted month in the regression). Appendix Figure E.1 shows the evolution of the exchange rate in the analyzed period. In Section 4.4, we explain that the effect of the depreciation does not pose a threat to our subsequent findings of the VAT holiday.

Figure E.34: Reporting of non-taxable sales and total sales in VAT return data



Notes: This figure shows the beta (β) coefficients of the following difference-in-differences specification $Y_{it} = \alpha_i + \gamma_t + \sum_{t \neq 2018} \beta_t D_{it} + \epsilon_{it}$ where i indexes retail subsectors and t denotes calendar years. The treatment indicator D_{it} equals one for grocery stores and zero for the other eight retail sectors (see Table A5). We estimate two separate regressions in which the outcome variable Y_{it} is the log of non-taxable sales (blue triangles) and the log of total sales (red squares), respectively. Semi-log coefficients are converted into percentage changes, and robust standard errors are computed using the Delta Method. Regressions are weighted by the number of VAT returns in each sector-year cell. The share of non-taxable sales over total sales for grocery stores was 2.4% in 2018.
Source: Authors' calculations based on ARCA's Statistical Yearbook (FY2015–2023), which reports disaggregated VAT return data for 230 retail sub-sectors.

Table A1: Temporary Value-Added Tax Cuts Aimed at Lowering Consumer Prices 2020–2025

Country	Temporary VAT cuts	Food Specific?	Yearly inflation	
			Food	All items
	(1)	(2)	(3)	(4)
Algeria	0%	Yes		9.3%
Angola	Cut 14% to 5%	Yes	24%	21%
Argentina	0%	Yes	35%	37%
Bahamas	Cut 10% to 5%	Yes		5.5
Bosnia and Herzegovina	Cut 17% to 5%	Yes	14%	6%
Bangladesh	Cut 15% to 7.5%	Yes (oil & biscuits)	8.6%	7.7%
Botswana	Cut 14% to 12%	No (standard rate)		12.4%
Bulgaria	0%	Yes	22%	15%
Congo DR	Cut 16% to 8%	Yes	6%	3%
Costa Rica	Cut 13% to 1%	Yes		8%
Ecuador	Cut 14% to 12%	No (standard rate)		3.7%
Fiji	0%	Yes	6%	5%
Jamaica	Cut from 16.5% to 15%	No		9.4%
Kazakhstan	Cut from 12% to 8%	Yes	23.1%	18.8%
Laos	Cut from 10% to 7%	No (standard rate)		39.3%
Morocco	0%	No (agricultural products)		8.3%
Mozambique	Cut from 17% to 16%	No (standard rate)		10.9%
North Macedonia	0%	Yes		18.7%
Oman	0%	Yes	4%	3%
Papua New Guinea	0%	Yes	9.9%	5.3%
Peru	0%	Yes		8%
Romania	Cut 9% to 5%	Yes	14%	16.4%
Russia	Cut 10% to 5%	Yes		11.9%
South Africa	0%	Yes	12.4%	7.2%
Tajikistan	Cut 15% to 14%	No		4.2%
Togo	0%	Yes	12%	8%
Tunisia	0%	Yes (coffee and tea)		10.1%
Turkey	Cut 8% to 1%	Yes	86%	72%
Uzbekistan	Cut from 15% to 12%	No (standard rate)		12.3%
Vietnam	Cut from 10% to 8%	Yes		4.5%
Zimbabwe	0%	Yes		243.8%

Notes: Column (1) lists the countries that recently cut their value-added tax rate. Column (2) indicates whether the VAT cut targeted food items. Columns (3) and (4) present food inflation and overall annual inflation, respectively. Yearly inflation corresponds to the year 2022 (relative to 2021) for all countries except for Argentina (which corresponds to the value observed between July 2018 and 2019, right before the reform we study).

Sources: IMF and the Argentine National Statistics Office (INDEC, for its acronym in Spanish) for inflation data. The information about the value-added tax cuts on basic food corresponds to the aftermath of the COVID-19 pandemic and was retrieved from VatCalc.com and online sources.

Table A2: Description of Data Sources

	Scanner Data			VAT returns	Expenditure Survey	CPI Prices
	All country	**Periferia Region**	**Córdoba Region**		**ENGHo**	
Purpose	Main sample	Substitution effects	Income/GE effects	Effect on govt revenue	Welfare estimates	Robustness
Coverage	National - All regions (5)	Only *Periferia*	Only *Córdoba*	National	National (except rural)	National
Period	2018-2021 (Jun)	2018-2022	2019-2020	2015-2023	2017/18	2017-2018
Frequency	Weekly (Chains) Monthly (Independents)	Weekly	Monthly	Annually	–	Monthly
Unit	Barcode–Region–Store-type	Barcode	Barcode–Store Id	Subsectors	Household–product	Subcategories
Products	Food	Food Beverages Non-food	Food Beverages Non-food	All purchases	All purchases	All purchases
Store type	Chains Independents	Chains	Chains Independents	All places of purchase	All places of purchase	All places of purchase
Source	Scentia Market company	Scentia Market company	Scentia Market company	ARCA National Tax Agency	INDEC National Statistical Office	INDEC National Statistical Office
Availability	Purchased	Purchased	Purchased	Publicly available	Publicly available	Publicly available

Notes: This table summarizes the data sources used in the analysis and some of their features. Scentia scanner data record both prices (unit values) and quantities sold for each cell; we use prices for the pass-through analysis and quantities for the quantity-response and income-effects exercises.

Table A3: Summary Statistics at Baseline (July 2019).

Number of Barcodes and Price Differences Across Store Types

		Stores				
		All stores	Chain stores	Independent stores		
Treated	All goods	3,562	1,934	2,563		
	No caps	471	265	343		
	Caps	3,091	1,669	2,220		
	No overlap	3,307	1,444	1,973		
	Overlap	915	915	915		
			Price difference		Barcodes	Observations
			-0.029***		915	5,696
			(0.005)			
Control	All goods	4,103	2,498	2,934		
	No overlap	3,598	1,852			
	Overlap	1,217	1,217			
			Price difference		Barcodes	Observations
			-0.042***		1,217	7,404
			(0.005)			
All goods	All goods	7,832	4,530	5,613		
	No overlap	6,905	3,296	3,885		
	Overlap	2,132	2,132	2,132		
			Price difference		Barcodes	Observations
			-0.036***		2,132	13,100
			(0.003)			

Notes: This table presents the *unique* number of barcodes (note that a given barcode may appear multiple times as it can be purchased in different store types s and regions r). Also, a certain barcode can belong to the overlap group in one region, e.g., GBA, and to the no-overlap group in another, e.g., Córdoba. For the price difference results, we show the β coefficient of the following regression: $log\ P_{i,s,r} = \alpha_{i,r} + \beta\ Independent\ Stores_s + \epsilon_{i,s,r}$, where $log\ P_{i,s,r}$ is the log of the observed price of barcode i, in store type s in region r the month before the reform (t_{-1}). α are barcode-region fixed effects while *Indepedent Stores* is a dummy that identifies those store types. Note that we run this specification the month prior to the reform for those overlapping goods, i.e., barcodes that are sold in chain and independent stores at the same time in a given region. As opposed to most of the regressions, in this one we use the log of the *observed* price instead of the *normalized* to the pre-reform one. Standard errors clustered at the barcode level in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table A4: Robustness: Pass-Through Across Sample Partitions

Regions	Frequency	Stores	Goods	Estimates: Removal	Estimates: Re-intro	Number of: Barcodes	Number of: Observations
Andina	Monthly	All stores	All goods	-10.618***	-0.162	7,250	505,761
				(0.655)	(1.776)		
Cordoba				-10.249***	-0.025	7,615	515,967
				(0.653)	(1.753)		
GBA				-10.568***	-0.125	6,976	475,430
				(0.651)	(1.809)		
Litoral				-10.516***	-0.742	7,117	478,681
				(0.686)	(1.774)		
Rest of pba + south				-9.930***	0.198	7,166	472,621
				(0.648)	(1.793)		
All	Monthly	All stores	T: Oils	-10.551***	-0.303	7,732	603,111
				(0.598)	(1.638)		
			T: Rice	-10.563***	-0.750	7,620	596,905
				(0.607)	(1.636)		
			T: Sugar	-10.343***	-0.483	7,766	609,138
				(0.595)	(1.592)		
			T: Canned fruits and vegetables	-10.741***	-0.457	7,190	570,947
				(0.635)	(1.830)		
			T: Eggs	-10.242***	0.099	7,599	602,600
				(0.603)	(1.652)		
			T: Fluid milk	-10.080***	0.345	7,764	606,541
				(0.588)	(1.627)		
			T: Batter and breadcrumbs	-10.359***	0.053	7,661	602,117
				(0.605)	(1.654)		
			T: Dried pasta	-9.892***	2.832*	6,876	545,734
				(0.663)	(1.343)		
			T: Tea and yerbas	-10.369***	-2.496*	7,095	543,883
				(0.564)	(1.458)		
			T: Yogurts	-10.552***	-0.714	7,570	584,403
				(0.630)	(1.718)		
			T: Sliced bread and lactal bread	-10.422***	0.045	7,550	597,107
				(0.612)	(1.681)		
All	Monthly	Large chains	All goods	-14.696***	-2.885**	4,530	319,896
				(0.572)	(1.289)		
		Independent		-6.194***	2.530	5,613	292,219
				(0.702)	(2.013)		
All	Monthly	Large chains	All goods	-14.696***	-2.885**	4,530	319,896
				(0.572)	(1.289)		
	Weekly			-14.571***	-2.479*	4,530	2,451,862
				(0.523)	(1.311)		

Notes: Each row reports the collapsed difference-in-differences pass-through estimate for a different partition of the sample, leaving out one region, one treated-goods category, one store type, or one data frequency at a time. The specification is identical to Table 3. Standard errors clustered at the product–region level (category × region) are in parentheses; significance stars are based on the wild cluster bootstrap (Cameron–Gelbach–Miller, 999 replications) at the product–region level, the same inference used in Table 3 and the event-study figures. For the weekly row, where the wild bootstrap is computationally infeasible on the larger sample, the standard error and stars are based on the analytic product–region cluster-robust t-test. ***, **, and * indicate significance at the 1%, 5%, and 10% level.

Table A5: Retail Sector VAT Data - Year 2019 (in millions AR pesos)

Sector	**Description (retail sales in...)**	**N Tax Returns** (1)	**Taxable Sales** (2)	**Non-taxable Sales** (3)	**Output VAT** (4)	**Input VAT** (5)
471	Grocery stores	26,781	793,372	26,533	155,764	137,282
472	Food products, beverages, and tobacco in specialized stores	35,148	319,667	13,999	54,809	48,771
473	Fuel for motor vehicles and motorcycles	4,797	476,836	43,113	99,356	84,021
474	Computer accessories and software; telecomm. equipment in specialized stores	4,170	55,971	451	9,675	7,270
475	Household equipment (textiles, hardware, electronics) in specialized stores	33,430	700,968	7,592	144,142	113,497
476	Cultural and recreational goods (books, magazines) in specialized stores	7,191	67,312	6,546	14,083	10,805
477	Other products (apparel, footwear, pharmaceutical, etc) in specialized stores	60,615	517,355	279,653	106,215	82,289
478	Mobile stands and markets	447	2,003	84	386	350
479	Not carried out in shops, stands, or markets (online, vending machines, etc)	3,750	24,234	331	4,863	4,066
Total	Retail sector	176,328	2,957,719	378,302	589,294	488,351
	All sectors (universe)	978,908	27,571,390	2,844,143	5,289,592	4,373,756

Notes: This table presents an extract of VAT administrative data for 3-digit retail sectors in the year 2019. The federal collection agency, ARCA, computes these aggregates based on microdata from VAT returns F.731 and F.2002. Values are expressed in millions of Argentine pesos. The number of tax returns corresponds to the monthly average. *Source*: Tax statistical yearbooks from the federal collection agency ARCA (calendar year 2019).